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American Anthropologist

NEW SERIES

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No. 1

FURTHER CONSIDERATION OF THE OCCURRENCE OF HUMAN REMAINS IN THE PLEISTOCENE DEPOSITS AT VERO, FLORIDA

By OLIVER P. HAY

IT is here taken for granted that anybody who is sufficiently interested to read this article has either already studied the papers written on the subject by Sellards, MacCurdy, Vaughan, Hrdlička, R. T. Chamberlin, and Hay, or that he will proceed to do so. This course will obviate the necessity of re-describing the topographical, geological, and paleontological situation at Vero.

After a study of the problems presented at Vero, the authors just named each expressed his independently formed opinion in a symposium which appeared in the January (1917) number of the *Journal of Geology*. Since that time Doctors Sellards and MacCurdy have contributed interesting articles on the subject¹ and the writer has published a condensed statement of his views.² It is to be regretted that Dr. Hrdlička has not, up to the time of writing these lines, been able to present his projected detailed report³ in which were to be discussed the broader anthropological and archaeological problems concerned in the matter.

At any rate, it seems to the writer that progress is being made;

¹ *American Anthropologist* (N. S.), vol. 19 (1917), pp. 239-261.

² *Journal Washington Academy of Sciences*, vol. VII, pp. 358-360.

³ *Symposium*, p. 50.

that some questions are already settled and others well on the way. The writers are agreed that there are presented at Vero three distinct geological deposits of Quaternary age; that in all of these there are found remains of animals which have hitherto been regarded as belonging exclusively to the Pleistocene; that human bones and artifacts occur in the middle and the uppermost of the three strata in some sort of association with remains of these Pleistocene vertebrates, and that the human bones and the artifacts differ little, if at all, from those of pre-Columbian Indians.

It appears further that the following questions are still matters of contention:

1. Are the vertebrate fossils found in deposits no. 2 and no. 3 there by primary inclusion, or have they been washed in from older deposits?
2. Were the human bones introduced into the two deposits by artificial burial or did they reach their recent positions by natural means?
3. Have any of the human remains found in no. 2, the age of the deposit itself?
4. What is the geologic age of stratum no. 2?
5. What is the geologic age of stratum no. 3?

1. Dr. R. T. Chamberlin¹ has taken the position that the vertebrate fossils in no. 2 and no. 3 have been washed in from older beds and within comparatively recent times. Not far back from the valley where these deposits occur, he found a stratum from 2 to 4 feet thick consisting of a "dark-brown to true-black, firmly indurated sand, or sandstone, cemented by ferric hydroxide and organic matter." Above this there is a soft, spongy, peaty layer, the remains of an ancient bog, which varies in thickness from nothing to 6 inches. These old bog materials are covered up by wind-blown sands to a depth ranging from 7 feet on the eastern border to hardly 2 feet one mile further west. Chamberlin has expressed the opinion that it is this "Pleistocene bog accumulation and its associated deposits which originally housed the old mammalian bones."

It seems to the writer that Sellards's last paper² disposes of this theory. The main canal and its two laterals gash in three

¹ *Symposium*, p. 36.

² *American Anthropologist*, vol. 19, pp. 239-251.

different directions the deposits noted by Chamberlin; and the banks and dumps of these canals and of the old drainage channels have been thoroughly searched and no fossils have been found in that old bog. This absence of fossils is remarkable because some bones and teeth might well have been expected to occur there. In connection with this dearth of bones in the bog, must be taken into consideration the fact that only those fossils, if any had been present, which had been eroded out of the bog materials along the banks of those little rivulets could have been swept down into the deposits of no. 2 and no. 3; for the remainder of the area has always been protected by the stratum of wind-borne sands.

Chamberlin has found in the deposits just mentioned what he regards as portions of the indurated sandstone of the old bog which have been carried down by the streams and so rolled and eroded that some of them are now designated as "cannon-balls." Now, if those vertebrate fossils had originally been buried in the peat of the bog they would have been at the best rather soft and brittle. What chance then would they have had for arrival at their destination in any recognizable condition when blocks of indurated sandstone were rolled into "cannon-balls"? If the bones had been buried in the indurated sandstone they would have been penetrated by the indurating materials, which is not the case; and some of them would now be found with portions of the hard sandstone adhering to them, which also is not the case. Most of the bones are broken and have suffered some decay, and some have been eroded; but most of them show no erosion whatever. It would, too, be just as reasonable to insist that the human bones had been washed down out of the bog as that this had happened to the bones of the animals.

Attention has been called by Sellards to a number of cases in which several bones of one individual were associated. It is perfectly obvious that, if even a complete skeleton of an animal had been buried in a deposit, had there lost its ligaments and the animal matter, and had become partly fossilized, and then disturbed and transported, we may say even a quarter of a mile, the chances would be almost infinitely small that any two bones would find

lodgment near each other. Yet of the wolf (*Canis ayersi*), thirty or more bones were found at one place; and near by was the skull and femur of probably the same individual. Many bones of *Chlamytherium*, apparently belonging to one individual, were found at one place. Several bones of the jabiru (*Jabiru weillsi*), probably of one bird, were found close together. A jaw and large parts of the tusk of a mastodon were found in close proximity. Let us imagine a partially fossilized tusk of a mastodon or a delicate skull of a tapir being rolled a quarter of a mile down stream intact, while blocks of indurated sandstone are being ground down into globes. Thin turtle shells, softened by decay of the animal matter, could hardly endure transportation without destruction.

The cases just mentioned concern more especially materials found in no. 2. The writer has mentioned the fact that seven bones of an extinct snapping turtle (*Chelydra sculpta*) had been found together in no. 3. An illustration of these will be published in the forthcoming report of the Florida Geological Survey. Recently the writer has studied some deer bones found in no. 3. At one of the stations on the south bank of the canal and extending along it about 15 feet, were collected fifty deer bones; and while many of these are broken and some are injured by decay, none show signs of wear by water and sand. In this lot there are at least three individuals; but who can suppose that, if a skeleton of a deer had been buried in the bog already referred to, and had then been washed out, probably little by little, and swept down stream, any two bones would have been landed within fifteen feet of each other? The fifty bones of the collection would have represented as many individuals. Among these bones is a part of a left maxilla with three molars which fit accurately against the molars of a lower jaw. A second and a third dorsal vertebra seem certainly to have belonged to one deer. Three lumbar, the third, fourth and fifth, fit nicely together. A sacrum is complete and both innominata are present and join it accurately. Two radii belonged certainly to one young deer. One individual of the lot, probably all of them, belonged to an extinct species, *Odocoileus sellardsiæ*.

Chamberlin¹ in referring to similar cases, suggests that such

¹ *Symposium*, p. 39.

remains may have been buried along the banks in the upland deposit and, on being undermined, have fallen down and been reburied without suffering transportation. Unfortunately for this explanation, fossil skeletons do not appear to be any more abundant in that upland deposit, along the Van Valkenberg valley than they are in the old bog further up the tributary streams.

The writer believes that the theory of the secondary deposit of the fossil bones and teeth in the strata no. 2 and no. 3 is untenable and that we shall hear little more of it.

2. Let us now consider briefly the question whether or not the human bones had been introduced into the deposits no. 2 and no. 3 by burial at human hands. The writer believes that he is correct in saying that only Dr. Hrdlička has expressed an opinion to that effect. It is evident that Dr. MacCurdy does not share this opinion; for in his article in the *American Anthropologist*, on page 258, he indicates his belief that we can be sure that the oldest human skeleton and cultural remains at Vero are as old as the base of the upper deposit. It is the opinion of the writer that Dr. Sellards, in his last paper, has put the question of intentional burial of those human bones out of dispute.

It is not for us to assign limits to the wisdom of God, nor to the cunning devices of nature in the accomplishment of her ends, nor to the foolishness of men; but we do nevertheless seek for some gleams of reason in what human beings do. Can anyone now explain why savages who make pottery and flint implements and bury their dead would dig a grave in a water-soaked bed of muck, where masses of leaves and sticks and stems and even logs were likely to be encountered; when, by going a few rods away, they could make the grave in loose dry sand? And, if the burial be granted, how did it come about that the bones of the skeleton, even the elements of the skull, became scattered so widely? Why, too, did they in their migrations so generally seek the parting plane between the two strata? A sufficient explanation of what is found in the case of the human remains called no. II is that a skeleton or a part of one, probably in a more or less scattered condition, had been covered up in stratum no. 2, when this was being deposited,

not far above where the bones were recently found. After the bones had lain there long enough to become rotten, possibly somewhat fossilized, a freshet unearthed them and scattered some of them, including the thoroughly macerated and easily disarticulated skull, over the sloping surface of the bed of the stream. Apparently at once a lot of vegetable debris was thrown down on them, thus fixing their position. The writer believes that the theory of intentional burial of the bones in either no. 2 or no. 3 can not be defended.

3. Do any of the human remains which are found in no. 2 have the age of the deposit itself? The writer believes that the human bones are themselves as old as are the deposits in which they are found. This is the opinion of Dr. Sellards. Dr. Chamberlin says:¹

While this deposition of no. 2 was in progress the human bones are believed to have received their first and only burial in connection with the stream deposit.

Dr. MacCurdy, as already quoted, believes that all that we can be sure of is that the human remains date back to the interval separating the middle (no. 2) from the upper (no. 3) deposit.

It is believed by the writer that the bones found in no. 2 were covered in when the bed was being deposited because:

- a. They have been found in that stratum in two different cases.
- b. They are in practically the same state of fossilization as are the bones of the animals.
- c. Not the slightest evidence has been produced to show that they were put there by human hands.

The circumstances connected with skeleton no. 11 have been discussed by Sellards, Hrdlička, MacCurdy and just now by the writer. MacCurdy in his last paper (p. 258) thought that Sellards in his paper of the same date had ignored this skeleton no. 1; but that is not wholly correct. While Sellards did not discuss this skeleton, he did refer (p. 248) to what he had already written about it. As to this skeleton nobody denies that the bones were found where reported. Nobody has attempted to show that there was any disturbance of the sand and marl above the bones; although the block of marl rock which capped the supposed grave was yet

¹ *Symposium*, p. 37.

there when the geologists and anthropologists visited the place. If, however, the anthropologist¹ believes that muck and sand, disturbed in digging and refilling a grave, will in time rearrange themselves so that the distinction between the two would be impossible, he would see little use in searching for a disturbance. Nobody has tried to explain in what way most of both legs and a little of both arms of that supposedly interred body might have been preserved and all the rest of the skeleton have disappeared. Had the skull, the vertebrae, the scapulae, and the pelvis been buried with the arms and legs, it is probable that some of them, or parts of them, would have been found either in the sand bed or lying loose at its base.

4. What is the geologic age of stratum no. 2? Before an answer is given to this question it will be necessary to discuss somewhat the geology and paleontology of the Pleistocene of North America.

It is now generally recognized by geologists that there were during the course of Pleistocene times four or five glacial and, corresponding to these, three or four interglacial, stages. These stages are presented in the following table, the names of the interglacial stages being italicized. A postglacial stage has been called by the writer the Wabash.²

10. *Wabash*.
9. Wisconsin.
8. *Peorian*.
7. Iowan.
6. *Sangamon*.
5. Illinoian.
4. *Yarmouth*.
3. Kansan.
2. *Aftonian*.
1. Nebraskan.

During the various glacial stages widespread deposits of till, with intermingled sands and gravel and boulders of northern origin, were laid down over portions of the Northern States. The accompanying map (fig. 1), the publication of which is permitted by the

¹ *Symposium*, p. 48.

² *Smithsonian Miscellaneous Collections*, vol., LIX no. 20.

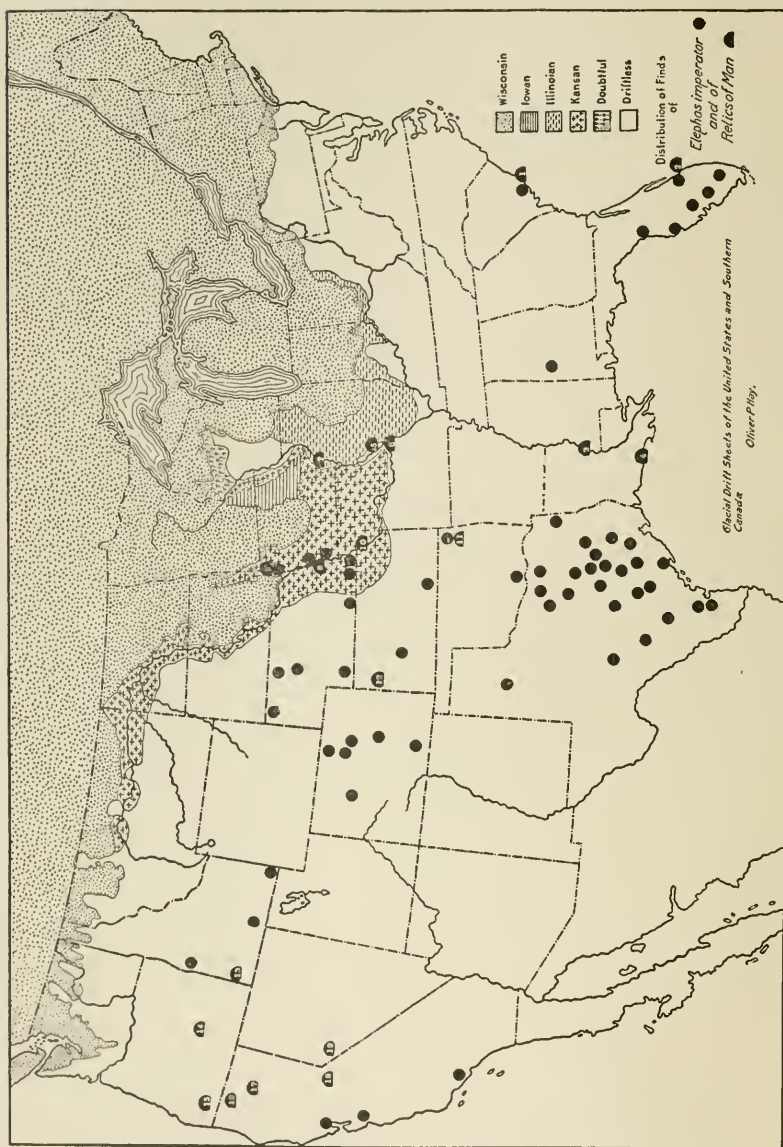


FIG. I

Carnegie Institution of Washington, shows the general distribution of each of these glacial drift sheets, except the Nebraskan. This is hidden by later deposits, except where the latter are cut through by rivers. The interglacial stages are represented here and there in the region by deposits of various kinds, river gravels, sands, old soils, and beds of loess. All of these sometimes bear fossils and thus give us glimpses of the life of the times.

The most important of the fossil-bearing deposits found intercalated between deposits of glacial drift are the Aftonian gravels and sands of western Iowa. The Missouri river in this region has cut down through the Kansan drift so as to expose the Aftonian and Nebraskan beds. From this Aftonian, the earliest interglacial Pleistocene, there has been collected an important assemblage of fossil vertebrates. A list of the species has been published by the writer.¹ It includes three species of mastodons, three species of elephants, four or five species of extinct horses, a hipparion, an extinct bison, at least two undetermined species of camels, the Canadian beaver, the giant beaver, peccaries, mylodon, and megalonyx. In what have been known as the Equus, or more properly the Sheridan, beds in western Nebraska and Kansas an identical fauna has long been known. In the publication just cited and on the same page the writer has published a list taken from Dr. W. D. Matthew, of the species found at the type locality of the Sheridan beds, near Grayson, some miles from Hay Springs, Nebraska. To this list are to be added two horses and *Elephas imperator*.

There can be no doubt that the Aftonian interglacial deposits are simply a continuation eastward of the so-called Equus, or Sheridan, beds.

If, now, we study the collections made in the drift-covered regions east of the valley of the Missouri river, we must recognize that the mammalian fauna had at the time of its existence here undergone some changes. In northern Missouri and in southern and eastern Iowa there are large tracts which are covered by Kansan till or by this overlain by loess of different ages. In

¹ *Iowa Geological Survey*, vol. XXIII, p. 26.

Illinois, Indiana and Ohio, there are extensive regions which are covered up by Illinoian drift or by more recent interglacial deposits. Many of the animals which are found in the Aftonian and Sheridan deposits are found on or in some of the younger deposits just mentioned, such as *Elephas columbi*, *E. primigenius*, *Mammut americanum*, several species of *Bison*, *Tapirus*, *Cervalces*, *Myiodon*, *Megalonyx*, *Castoroides* and *Equus complicatus*; but no *Elephas imperator* has been found, no species of *Hipparion*, none of the many Sheridan and Aftonian horses except *E. complicatus*; no saber-toothed tigers and no camels. The region and the climate must have been favorable for their existence; and they would have left some traces of themselves if they had existed. From our present knowledge we can only conclude that they had perished.

When we come to examine the rather extensive collections which have been made in the deposits of the old filled-up lakes which lie upon the last drift sheet, the Wisconsin, we find that the vertebrate fauna had suffered further extensive changes. There are yet the two elephants and the mastodon, peccaries, the existing moose (*Alces*) and the extinct moose (*Cervalces*), also *Castoroides*, *Megalonyx*, and short-horned bisons; but there are no longer found any of the long-horned bisons of former times, no tapirs, no mylodons, and of course, none of the species which had dropped out of existence shortly after the first interglacial stage. It cannot be affirmed that the climate of our northern states did not permit the presence there of the missing species; for there was a period after the passing away of the Wisconsin ice sheet when peccaries lived along the shore of Lake Erie and in Michigan; and the megalonyx penetrated as far north as Minneapolis. What had taken place is what might have been expected; namely, that as Pleistocene times passed on one species after another dropped out of existence, while its place was either taken by other species or probably in most cases left vacant.

It may be well to take into consideration here the percentage of extinct species which are found at some of the places where considerable collections have been made. Of the species found in the Aftonian of Iowa about 90 per cent. are extinct; but as the deposits

are usually coarse, showing rapid currents, few of the smaller animals, such as rodents and insectivores, have been collected. Only the larger mammals are represented. The type locality of the Sheridan beds, at Grayson, Nebraska, has furnished about 71 per cent. of extinct forms. The cave at Port Kennedy, Pennsylvania, whose deposits are assuredly of early Pleistocene age, presents a fauna of which 80 per cent. of the species are no longer living. The conditions there were apparently favorable for the preservation of all kinds of land vertebrates; and the percentage may be taken as representing in a general way the composition of the early Pleistocene vertebrate fauna, as regards species yet living and those which have become extinct.

In a fissure in northwestern Arkansas, Barnum Brown secured an important collection of vertebrates. Of the mammals about 47 per cent. belong to extinct forms. On account of this low percentage, the absence of various species, and the presence of some forms of aboreal type, the writer concludes that the collection represents the Illinoian stage; but inasmuch as many low and small forms not so likely to be distinct from living species are present and many extinct species are missing which certainly were then living, it seems not impossible that the percentage is too low to represent the actual fauna. Naturally, after the Wisconsin stage had passed, relatively fewer extinct species appear and finally the fauna passes into that of the Recent epoch.

At this point reference may be made to the discussion which went on about the time (1890) when G. K. Gilbert published his monograph on Lake Bonneville. Cope and Marsh at that time held that the so-called *Equus* fauna and the deposits which contained it belonged to the Pliocene.¹ Gilbert, basing his views especially on the physical evidences, came to the conclusion that the fossil-bearing deposits around Lake Lahontan, Lake Bonneville, and Fossil Lake, Oregon (the latter furnishing what Gilbert accepted as a typical *Equus* fauna), belonged to the Pleistocene. This view has been accepted by vertebrate paleontologists in general. Gilbert, however, went further. Holding then that there had been

¹ Gilbert, *op. cit.*, pp. 397-400.

only two glacial stages and finding that the old lake deposits which furnished the *Equus* fauna belonged near the close of the later stage, he assigned the beds to the late Pleistocene. In this the writer believes that Gilbert was wrong. The fauna of Fossil Lake is identical with that found near Hay Springs, in the Sheridan beds. These beds, followed north, were found by W. B. Scott to pass beneath a drift sheet, which can be only the Kansan; while toward the east they pass likewise beneath the Kansan and reveal the same fauna in the valley of the Missouri river. Essential elements of it never reappear in deposits overlying the Kansan and later drift sheets. Between Gilbert and the paleontologists mentioned the honors were about equally divided.

Returning to the region of the Great Plains and making our way into Oklahoma and Texas, we meet everywhere with the Aftonian, or Sheridan, fauna. *Elephas imperator* (fig. 1), camels, several species of horses, some diminutive, others gigantic, tapirs and long-horned bisons, are found or likely to be found anywhere, even down to the border of the Gulf. Toward the east, camel remains have been found on Brazos river in Austin county, Texas; and a part of a skeleton of *Elephas imperator* was found by Dr. T. Wayland Vaughan close to the line between Texas and Louisiana. In some places this Pleistocene fauna is associated with remains of a species of mastodon belonging to *Gomphotherium*, a genus which was abundantly represented during the Pliocene. The genera *Mylodon* and *Megalonyx*, those old invaders from South America, have their ranks strengthened by the presence of *Megatherium*, *Nothrotherium*, and *Glyptodon*. It is surprising in studying the Pleistocene vertebrates of Texas how seldom remains of yet living species are met with.

In the low region along the lower portion of the Mississippi river from Louisiana to Alabama there have been found up to this time no *Elephas imperator*, no camels, no glyptodons, and only two species of horses. The fauna so far secured is approximately that of the post-Illinoian. The only exception to this is the association of *Equus leidy* with *E. complicatus* in deposits at Natchez. Here too, is found a saber-toothed tiger, a species of which occurred

also in the Arkansan fissure already mentioned. Inasmuch as the muskox *Symbolos cavifrons* has been reported from the same place it may be that the age of the deposits is Illinoian.

When we enter Florida the fauna which has been described as occupying the Aftonian beds (the so-called Equus beds of the Great Plains) and the coastal plain of Texas, reappears in full strength, but with slight modifications. There are present of the *Xenarthra*, *Megatherium*, *Mylodon*, *Megalonyx* and *Glyptodon*. *Chlamytherium* replaces *Nothrotherium*; and *Dasyfus* is present. Of horses there are three distinct species, a large one, a medium-sized one, and a small one. Tapirs are represented by probably two or three species. Peccaries, hitherto unknown in the Gulf States, come to light at Vero. Camels are not abundant in the state, but are there. The genus *Bison* appears to have been represented by *B. latifrons*, the one with the longest horns, and probably by other species. Besides the common mastodon and the Columbian elephant, *Elephas imperator* has been found in several places. Great wolves and coyotes and foxes of extinct species are recognized. In addition to these there are many smaller creatures, some now extinct, many yet existing. There were many tortoises mostly belonging to extinct species and some of them were of immense size.

No one, it seems to the writer, can doubt that the fauna which has been described lived in the region west of the Mississippi in early Pleistocene times. There appears to be no good reason to doubt that it lived in Florida at the same time. Did it, now, continue to live there until quite recent times? Our friends the anthropologists appear to be willing to believe this. The writer holds that the view is wholly wrong.

a. It is improbable that a fauna composed as this was of, first, a native element, of another which had reached the country by way of the cooler regions of Asia, and of a third which had made an irruption from various parts of South America, a mixed assemblage of contending elements, subjected to the extreme climates of glacial and interglacial epochs and repeatedly to the pressure of tribes forced down from the north during glacial times, that such a fauna

would continue practically unchanged up into the Recent period and then suffer sudden extinction.

b. If species of *Megatherium*, *Myiodon*, *Chlamytherium*, *Glyptodon*, three species of horses, two or more species of tapirs, camels, species of long-horned bisons, *Elephas imperator*, and saber-toothed tigers lived in Florida after the close of the Wisconsin glacial stage, why did not some of these extend their range north as far as the northern half of Ohio, Indiana, and Illinois? Nearly all of them had at one time lived as far north as those states, and evidently the climate invited them; and the Wisconsin drift would not have burned their feet so soon after the ice had left it.

c. Why also did not these animals occupy even the lower part of the valley of the Mississippi? Along this river throughout Louisiana is found a fauna which was appropriate to about the middle of the Pleistocene; but the elements which distinguish the early Pleistocene are missing. If they were living so late as the Recent epoch in Florida their absence in Louisiana is astonishing. The answer to these problems is that the extinct animals so often mentioned did not live on up to the Recent epoch.

For the sudden wiping out of a great assemblage of such strange beasts as existed in Florida during the Pleistocene, the rapid deposit of sand and vegetable debris mingled with trunks of trees and the bones of animals and of men, and that at as late a period as 4000 years ago, the writer can think of no adequate agency except the Noachian deluge. According to the marginal readings of the authorized version of our Bible this event occurred 2349 years before Christ; that is, 4266 years ago. Archbishop Usher, a very learned man, was perhaps tainted a little by advanced ideas and was somewhat too liberal in his dispensing of time.

4. It appears to be possible at this point to answer the question asked on page 7: What is the geologic age of stratum no. 2? In view of the fact that about 70 per cent. of the mammals found in that stratum are extinct, and that these include species which belong with the fauna of the Sheridan and Aftonian beds and are not known to have existed at a later time, the stratum is regarded as belonging to the first third of the Pleistocene.

5. What is the geologic age of stratum no. 3 at Vero? In this stratum, known as the muck-bed, there is a lower percentage of extinct mammals, about 50 per cent. This appears to indicate a somewhat later time, about the middle of the Pleistocene. This lower percentage may, however, be due partly to what one may call accidental causes. The geologists regard the interval between the two deposits as having been brief.

Dr. Hrdlička¹ argued that a state of culture as advanced as that shown by the artifacts found in the deposits at Vero implies a numerous and widespread population and that such a population would surely have left many tangible traces of its presence on the continent, some of which at least would have been discovered by this time. Dr. MacCurdy² has expressed practically the same opinion.

The present writer is wholly willing to say that the conclusions reached by these distinguished anthropologists are very reasonable; furthermore, that the requirement which they make need not go entirely unsatisfied. The following cases are contributed; but they have long been accessible to everybody, and it is needless to say that our good friends the anthropologists have set up over most of them the danger signal. Let us set out from the Atlantic coast.

1. In the *Proceedings of the Academy of Natural Sciences* for 1859 (pages 178-179) Prof. F. S. Holmes gave an account of the exhumation of some bones, teeth and a nearly entire tusk of a mastodon near Charleston, South Carolina. These remains were found in sands underlying peat in draining a large swamp and at a depth of about 3 feet from the surface. Professor Holmes, with a small party of gentlemen, was present during the exhumation, and Holmes found immediately alongside of the tusk a fragment of pottery. This was said to be similar to that manufactured at the present time by the American Indians. Holmes stated that bones of a deer and two teeth of a horse were also found.

Naturally, somebody will say that this potsherd had reached its position by some accidental means. It might appear that a

¹ *Symposium*, p. 50.

² *American Anthropologist*, vol. 19, p. 261.

burrowing muskrat had been responsible; but, although muskrats were in that region in Pleistocene times, no muskrats have in Recent times been found anywhere near Charleston. The simplest and most probable explanation is that the pottery was dropped near that tusk about the time that the animal died. The presence of the horse teeth mark the time as being not later than about the middle of the Pleistocene. In similar deposits about Charleston have been found remains of tapirs, peccaries, mylodon, megatherium, elephants (among them *Elephas imperator*), hipparion, extinct capybaras, and probably camel. These indicate early Pleistocene age.

2. The discovery of human remains and artifacts at Vero naturally takes its place here.

3. In 1846¹ Dr. M. W. Dickeson exhibited before the Philadelphia Academy a large series of fossil bones which had been obtained by him at Natchez, Mississippi. Among these were the head of a megalonyx and parts of the skeleton of others, besides remains of a bison, an extinct bear, a horse, and apparently a mylodon. These had been found in a blue clay which underlies the loess of that region. Lyell, who afterwards visited the ravine in which the remains were found, stated that it was 60 feet deep. Dickeson, at the meeting referred to, exhibited also a part of a human innominate bone which he said had been taken out of the same blue clay, at a depth of 2 feet below the skeleton of the megalonyx. Naturally, efforts have been made to escape the conclusion that the human bone was as old as its apparent geological situation and its association with the extinct animals implied. Lyell thought that the innominatum had possibly fallen down the cliff from some Indian graves at its summit. Others have suggested that it had been washed down to that spot from some place farther up the ravine. The bone is yet preserved in the collection of the Philadelphia Academy; and it has been described and figured by Leidy.² He stated that it did not differ in any way in appearance and manner of fossilization from the megalonyx bones with which it was found.

¹ *Proceedings of Academy of Natural Sciences*, vol. III, p. 106.

² *Transactions of Wagner Institute*, vol. II, pp. 9-12, pl. II.

In 1892 Dr. Thomas Wilson¹ had his attention attracted to this human bone and proceeded to have a chemical analysis made of it and of a mylodon bone which had been left by Dickeson. Wilson stated that the color, texture, and general appearance of the mylodon bone had a remarkable similarity to that of the human bone. The analyses were furnished by Prof. F. W. Clark, of the U. S. Geological Survey, and they resulted in showing that the human bone had a content of over 22 per cent. of silica, while the mylodon bone had not quite 4 per cent. It would be interesting in this connection to have Indian bones from the summit of that cliff chemically analyzed.

A considerable number of species of extinct animals have been reported from the loess and deposits below it at Natchez, many of them apparently from the ravine mentioned above. In the opinion of the writer they indicate a middle Pleistocene period.

4. In 1866, Mr. J. F. Clew, of Petite Anse, Louisiana, called on Doctor Leidy and exhibited a piece of coarse matting which he affirmed had been found at Petite Anse, lying on the top of a newly discovered bed of rock salt, at a depth of about 16 feet from the surface, and immediately below some elephant bones. Clew further stated that there were in view other pieces of matting and other bones for anyone to see who would visit the place. Leidy tried to have the Philadelphia Academy send a competent man down there, but nothing was done in the matter.

Prof. Joseph Henry read a paper on the subject before the Chicago Academy of Science.² Nobody has ever questioned Clews's good faith in the affair. In 1890³ J. F. Joor examined an extensive excavation which was being made to reach the rock salt. He found great quantities of pottery in a swamp muck, which had a thickness of 10 to 12 feet; but the pottery appears to have been in hollows which reached down from 3 feet to 6 feet from the surface. He concluded that Indians had been accustomed to boil down the brine which formed a spring there. He found a single piece of cane basket in a lump of mud, and he thought that it came

¹ *American Naturalist*, vol. xxvi, pp. 628-631.

² *Transactions of Chicago Academy of Sciences*, vol. i, p. 233.

³ *American Naturalist*, vol. xxix (1895), pp. 394-398.

from the lower part of the muck, or loam, below the level of most of the other human vestiges.

Below the loam was found a blue clay 2 feet or more thick. In this at a depth of from 16 to 20 feet from the surface, Joor found great quantities of bones of animals. These were afterwards studied by Professor Cope.¹ It appears from his communication and other reports that the following animals have been found at this place: *Megalonyx jeffersonii*, *Myiodon harlani*, *Equus complicatus*, *Odocoileus virginianus*, *Bison* sp., *Mammut americanum* and *Elephas columbi*. There are here no species which compel us to refer the deposit to the early Pleistocene. *Myiodon harlani* and *Equus complicatus* forbid the reference to late Pleistocene.

It has been suggested that Indians of recent times had sunken a shaft to the rock salt. Of this there is no evidence; it is improbable; and, if true, it is improbable that the tradition of it would have been lost. It has been thought, too, that such things as the matting, the pottery, and the animal bones had been washed down from the surrounding hills within rather recent times and covered up in the valley; but, while the bones are often broken and sometimes partly decayed, they do not show the effects of transposition. An explanation that appears to meet the requirements of the case is that about the middle of the Pleistocene, the valley was cut down so as to expose the rock salt; that men, probably Indians, carried away salt in cane baskets; that, later, through some slight change in level, material began to be deposited in the valley; and that, while this was going on, the mylodons and other animals lived and died and left their bones there.

5. It does not seem to the writer that the account given by McAdams² of the finding of a stone ax in Walkerville township, Greene county, Illinois, ought to be treated as suggesting a conspiracy of liars. Taken by itself the reported discovery would naturally have little weight; but if cases of the kind were common, no hesitation would be shown in accepting it. Dr. Shimek's objection³ that there is no loess in that region which has a depth of 70

¹ *Proceedings of American Philosophical Society*, vol. xxxiv, pp. 458-461.

² *Proceedings of American Association for Advancement of Science*, vol. xxix, p. 720.

³ *Op. cit.*

feet is not decisive. Worthen¹ states that the loess on that county may be from 40 to 60 feet thick. Possibly McAdams was not an expert in identifying loess; and there are often differences of opinion among the experts. It is also possible that the well had penetrated the Illinoian drift and reached a still older deposit.

6. In the *Records of the Past*, vol. II, 1903, pp. 26-28, Cyrus A. Peterson, M.D., gave an account of the discovery of a stone ax in September, 1902, near Clayton, a village just west of St. Louis and nine miles west of the Mississippi river. This region is covered with a deposit of loess which at the place named has a thickness of from 10 to 15 feet.² During the construction of the belt line of railway around the city of St. Louis a member of the engineering corps picked up the ax in question after it had been turned up by the plow of the graders. The man in charge of the work removed the clay which enclosed the ax, and this was taken to his office and scrubbed clean. It then passed into the possession of the chief engineer. The ax was found at the juncture of the loess with the substratum of red clay, and at a depth of 14 feet from the surface. It appears not to differ from Indian axes of recent times. There appears to be no reason whatever for questioning the authenticity of the discovery.

7. In 1891 Prof. F. M. Witter, of the public schools of Muscatine, Iowa, read a paper³ in which he told of the finding of arrow points in the loess at Muscatine, Iowa. In one case a brickmaker reported that he had taken a chert point from the loess at a depth of 12 feet from the surface. At another time the same man took an arrow point from the same bank at a depth of 25 feet from the surface. Dr. B. Shimek⁴ in speaking of this case refers to it as "Witter's cautious references to the discovery, by another party, of arrow-heads under extremely doubtful circumstances." As a matter of fact Witter investigated the matter as well as probably any one could and he expressed no doubt as to the authenticity of the discoveries. There was a third arrow point taken by the same

¹ *Geological Survey, Illinois*, vol. III, p. 123.

² Fenneman, *U. S. Geological Survey, Bulletin* 438, p. 33.

³ *Proceedings of Iowa Academy of Sciences*, vol. I (1892), pt. 2, p. 66.

⁴ *Bulletin of Geological Society of America*, vol. XIX, p. 244.

brickmaker from another brickyard in which there was a blue clay whose upper surface was 8 feet from the top of the ground. This arrow point had passed through the mixing bed before it was discovered. Witter examined the bank and inquired into the circumstances and wrote "I believe with Mr. Freeman, that the arrow point must have come from the blue clay." There is no intimation whatever that he doubted the word of the brickmaker in respect to any of the discoveries. The loess has been regarded as a deposit of the Sangamon stage; but, inasmuch as an extinct reindeer, *Rangifer muscatinensis*, has been found it it, a cold climate is indicated. One might not be far out of the way to assign the time to the Iowan glacial stage.¹

In the publication of 1892 above referred to, Witter reported other artifacts found near Muscatine. About a mile above its mouth, Mad Creek had cut away a point of a hill, leaving a bluff almost perpendicular and about 40 feet high. The top was covered by loess. About 10 feet from the summit was a bed of gravel one foot thick, from which a student had taken a considerable part of a tooth of an elephant. Witter examined this bed and in doing so found in it numerous flint chips such, he adds, as are supposed to have been struck from arrow and spear points, knives, etc.

8. In his report on the geology of Pottawatamie county, Iowa, Dr. J. A. Udden² reported the alleged discovery of a stone ax in the loess at Council Bluffs. It was met with in tunneling for the cellars of a brewery, under 30 feet of loess and at a distance of 40 feet from the mouth of the tunnel. Udden stated that the ax had on one side an adhering incrustation of calcareous matter that had evidently been deposited by ground water. The ax had been discovered by workmen and immediately turned over to the engineer who had charge of the operations. It is hard to see here any motive for deception on the part of anybody. Udden found no evidences of disturbance of the loess at the place.

9. In 1876 Dr. Samuel Aughey³ wrote that he had found, three miles east of Sioux City, Iowa, a small arrowhead in the loess;

¹ Udden, *Iowa Geological Survey*, vol. IX, p. 360.

² *Iowa Geological Survey*, vol. XI, p. 261.

³ *Annual Report, U. S. Geological Survey of the Territories* (1874), p. 254.

but he did not furnish any details. He stated further that he had found, two and a half miles southeast of Omaha, in a railroad cut, at a depth of 20 feet in the loess, and at a distance of at least 6 inches from the edge of the cut, a large coarse arrow or spearhead. It is fair to say that Doctor Shimek¹ characterizes these statements as absolutely unreliable; but the reasons are not given.

10. In 1907² Miss Luella A. Owen reported the discovery of a chipped implement in the loess near St. Joseph, Missouri. The implement is described as being composed of a fine, close-grained trap-rock. It was taken out of a perpendicular wall of the loess, along a highway, by an attorney of St. Joseph who was interested in collecting such objects. It was firmly imbedded in the loess, with the point directed nearly downward, at a height of 10 feet above the road and not less than 20 feet below the natural surface. The one side turned somewhat upward was partly coated with oxide of iron. It might, of course, be asserted that at some time in the past there had been a crevice here into which the ax had fallen. The numerous applications of this explanation tend to show that Indian implements have contracted a habit of waiting for such opportunities.

11. An interesting occurrence of flint implements in association with remains of extinct animals has been described by Prof. W. H. Holmes, of the United States National Museum.³ From a sulphur spring not far from Afton, Oklahoma, Professor Holmes secured more than 800 specimens, including fragments of arrowheads, spear points, knives, etc.; and he estimated that as many more had been previously removed. This collection of beautiful flints is now in the National Museum. Nearly the whole lot was found piled together at the bottom of the spring at a depth of from 4 to 7 feet. In the same spring were found, according to Holmes' report, many teeth and bones of mammoth, mastodon, horses, buffalo, deer, elk, and wolf. The greater part of these were found within a radius of 3 feet from the spring basin, at a depth of from 4 to 7

¹ *Op. cit.*, p. 244.

² *Records of the Past*, vol. VI, pp. 289-292.

³ *American Anthropologist* (N. S.), vol. 4 (1902), p. 108. *Report of U. S. National Museum* (1901), p. 237.

feet. Within this area were found at least 100 mastodon teeth, 20 mammoth teeth, and many teeth of horses and bison. Professor Holmes came to the conclusion that Indians who had inhabited the region had regarded the spring as sacred and had cast the flint implements and possibly many of the bones and teeth into it as offerings to the divinities of the spring. It is to be noted that the presence of all these objects indicates that the vent of the spring had not changed position for a very long time.

The present writer has had the opportunity to study the collection of bones and teeth which was made at Afton, and he finds that it includes 19 distinct species of mammals. Among these are *Castoroides ohioensis*, four species of extinct horses, two species of camels, the extinct moose *Cervalces*, the extinct muskox *Symbos cavifrons*, *Elephas columbi* and *E. imperator*. Of the latter there are several beautifully preserved teeth. Including in the estimate the common bison and two wolves, which may have got into the spring within comparatively recent times, we find that two thirds of the species are extinct. In short, we have here again the Sheridan, or Aftonian, fauna which is so often met with all over the Plains region. That these remains were cast into the spring by human hands is out of the question; for such objects are undoubtedly very rarely found on the surface in that region; and they are, according to Holmes' investigations, met with in the deposits around and at a distance from the spring. Nor does the writer believe that those bones and teeth that were found in the spring belonged necessarily to animals that died within a very few feet of the spring.

It seems perfectly evident that a stream of water rising from the earth must continually wear away the walls of the orifice, which does not need on that account to become larger. The earth near by will be pressed toward the opening and will keep its size reduced. The effects of long-continued pressure of the earth on retaining walls may easily be observed. The result will be that there will be a slow but unceasing movement of the ground and whatever is in it toward the spring. Once within the spring, the fine materials will in time be swept away, while the bones and teeth, if any, will

remain there until they are worn out or decay. If that sand deposit at Afton was laid down during the first third of the Pleistocene, it is not strange that so many bones and teeth were found in the spring itself. Professor Holmes himself, in a way, recognized the possibility of this movement of the ground.¹

If we assume that the diameter of the vent was two inches and that a layer one half inch in thickness around the vent was carried away in a year, an estimate will show that in 100,000 years all the earth within a radius of about 30 feet would have been brought up to the spring and swept away.

As already said, Professor Holmes has regarded the flint implements found at Afton as the offerings of Indians to the god of the spring; and he has furnished some evidence in support of his view. It is impossible to refute this opinion; but the writer does not see why the testimony of superannuated Indians in this case should be regarded as having greater value than that of honest white men, in other cases. And there may be found some other explanation of this matter. Possibly early Pleistocene Indians, lying in wait to get meat for their families, shot arrows which missed the mark and fell into the muck. In time, like the teeth and bones, they were carried into the spring and during long ages accumulated there. They were sharpened to keen edges, perhaps not to touch the hearts of the divinities of the spring, but to pierce the tough hides of the buffaloes. Against the theory that the flints were thrown into the spring is the fact that one implement was found in the deposit of sand at a depth of 3 feet from the surface of the ground and 3 or 4 feet from the curbing which was put down in the spring. At any rate, we have here another intimate association of artifacts of human beings with remains of early Pleistocene mammals, and this may mean their contemporaneous existence.

12. In 1902² Dr. S. W. Williston reported that in 1895 Mr. T. Overton and Mr. H. T. Martin, assistants in the paleontological department of the University of Kansas, had discovered an arrow-head lying beneath the scapula of a fossil bison, in Logan county,

¹ *Op. cit.*, p. 242.

² *American Geologist*, vol. xxx, pp. 313-315.

Kansas. At this place the skeletons of five or six adult bisons were found within an area of about 10 feet square, besides the skeletons of two or three young ones. Overlying these skeletons was 20 feet of the so-called plains marl. Mr. Martin has informed the writer that the bank where the first of the bones was seen was perpendicular and that it was 12 feet farther in where the arrow-head was found. Dr. Williston had long known Mr. Martin, who first saw the weapon; and he had not the slightest doubt of the reliability of the testimony given by him and Mr. Overton.

Now, the fossil bisons found at that place belong to *Bison occidentalis*, a species which has left its remains at a good many localities in our country. None of these remains, however, has ever been reported from any deposit which overlies Wisconsin drift. The skeletons found in Logan county may therefore be as old as the Aftonian or may belong to any succeeding pre-Wisconsin stage.

13. A brief reference must be made to what is known as the Nampa image. In 1889 an artesian well was being bored at Nampa, Idaho, a town on the Oregon Short Line railroad. The well was closely tubed by a six-inch pipe. While materials were being brought up from a depth of about 320 feet, there appeared an image an inch and a half in length which represented a woman and which was made of baked clay. A brief account of this discovery is given in G. F. Wright's *Ice Age in North America*, page 701, with a figure of the object. In the *Proceedings of the Boston Society of Natural History*, volume XXIV, 1890, pages 424-450, appear the discussions of the investigations which were made on this image. The possibility of any accidental or fraudulent intrusion of the object appears to be excluded.

In arriving at the depth noted above, the drill passed through 60 feet of soil, then from 12 to 15 feet of lava rock; while below this were deposits of various kinds. Naturally our anthropologists are inclined to question the authenticity of this discovery, especially on account of the great age that it indicates for a rather advanced race of men. Wright accepts the object as genuine, but regards the deposits as rather late Pleistocene.

In the Nampa folio, no. 103 of the United States Geological Survey, a section of the well is given. Reference is made to the discovery of the image. The authors say that "further confirmation of this occurrence has not been forthcoming and the image may have been dropped into the wellhole by some one wishing to perpetrate a practical joke." This statement is to be compared with the discussion before the Boston Society as cited above.

The beds passed through in this well are referred by the authors of the folio to the Pliocene. From deposits within the quadrangle believed by them to be of the same age they reported remains of *Equus*, *Procamelus* (probably *Camelops*), *Mammut*, a mastodon which was different from *M. americanum* and which therefore probably belonged to *Gomphotherium*, besides some other apparently extinct mammals, and an extinct swan. Here we have once more the *Equus*, or Sheridan, or Aftonian fauna, and the deposits certainly belong to the Pleistocene.

14. Brief mention will be made of the supposed discovery of arrow and spearheads of obsidian at Christmas lake, Oregon, in 1877, as reported by Professor Cope.¹ At this place there was found an abundant fauna consisting of mammals and birds, and belonging, as Cope then held, to the Pliocene. Mingled with these remains were found numerous obsidian implements. Naturally, efforts have been made to show that these weapons were of recent origin; and Cope himself² admitted that the evidence was not absolutely conclusive. Nevertheless, the other view may well be the correct one. It appears not improbable that the bones and flints had rather recently been brought to the surface by the action of the winds.³

15. Attention may be called to an article written by Prof. J. F. Kemp, of Columbia University, in 1906,⁴ in which he details the finding of mortars and pestles in auriferous gravels at Waldo, Josephine county, Oregon. He stated that these occurrences add

¹ *American Naturalist*, vol. XII, p. 125.

² *Proceedings of Academy of Natural Sciences, Philadelphia*, 1882; *American Naturalist*, vol. XXIII, p. 979.

³ Gilbert, *Monograph XI, U. S. Geological Survey*, p. 394.

⁴ *Science*, vol. XXIII, p. 435.

their contribution to the general mass of evidence regarding the antiquity of men in the far west. He also expressed his realization of the criticism which is usually brought to bear on such reports.

16. In 1880¹ Professor Cope reported that during the summer of 1879 he had obtained bones of *Mylodon* from the auriferous gravels of Klamath river, near Yreka, California, from excavations which he had personally examined. He secured also vessels of vesicular basalt, which, he said, had been undoubtedly procured from the same excavations. Of course, there was some chance here for the perpetration of a joke on a paleontologist, but Cope was a man not easily imposed upon.

17. We ought not to pass by the discoveries made in the caves of northern California by Dr. John C. Merriam and his students, Dr. W. J. Sinclair and Mr. E. L. Furlong. These discoveries were discussed by Doctor Merriam and Prof. F. W. Putnam in the *American Anthropologist*, volume VIII, 1901, pages 221-228; 229-235. In Potter creek cave Dr. Sinclair found 45 species of mammals, of which at least 21 were considered extinct; that is about 46 per cent. The list was presented by Sinclair in 1904.² There are in it two species of horses, apparently two of *Megalonyx*, a bison, a camel, the remarkable artiodactyl *Euceratherium*, the extinct wolf *Canis dirus*, and the extinct bear *Arctotherium simum*. The fauna is regarded by Sinclair as of the same age as that of Fossil Lake, Oregon, and that found near Hay Springs, Nebraska.

Now, associated closely with these remains were many splintered bones, some of which seemed to have been shaped and polished by human hands. Regarding these Professor Putnam remarked that it seemed impossible to explain their presence excepting by the agency of man. Dr. W. D. Matthew of the American Museum of Natural History and J. W. Gidley examined these bones and reported that in their opinion the only explanation of the perforations was that they were the work of man.

In Samwel cave was found a fauna which was regarded as somewhat younger than that of the Potter creek cave. In it were found

¹ *American Naturalist*, vol. XIV, p. 62.

² *University of California, Publications in American Archaeology and Ethnology*, vol. II, p. 17.

split and polished bones similar to those mentioned; besides these were secured a chipped fragment of basaltic lava and a chipped fragment of obsidian. These were, however, not seen in place.

18. Table Mountain, in Tuolumne and Calaveras counties, California, has for more than a half century been a storm-center of debate. For information and opinions the reader may consult J. D. Whitney,¹ G. F. Becker,² W. H. Holmes,³ W. J. Sinclair⁴ and G. F. Wright.⁵ The contention regards the alleged discovery of remains of man and his works in the auriferous gravels, and the age of the remains. Whitney, Becker, and Wright affirm the sufficiency of the evidence; Holmes and Sinclair deny this. The two latter writers certainly have shown great skill in throwing doubt on all the finds. Fortunately, all the authors agree that Table Mountain is a reality.

Professor Holmes appears to base his objections to admitting the authenticity of the discoveries on the idea that they would prove the existence of a race of Tertiary Indians as advanced as those of today. This view is certainly incorrect. The mammals that have been mentioned in connection with the discoveries in that region are rhinoceros, elotherium, hippopotamus, mammoth, mastodon, tapir, camel, horse, bison, and deer. Certainly the first two do not belong with the others, as Cope long ago pointed out; and they are not known to have had anything to do with any human remains. As to the hippopotamus, there was some error. The existence of any animal of the kind in this country has not been confirmed. The other animals are members of the Sheridan fauna so often mentioned in this paper; and, so far as we know, it existed during the first interglacial period of the Pleistocene. If these human remains and artifacts and those animals have been found in those auriferous gravels, as the writer believes the evidence taken as a whole indicates, we do not have to accept a Tertiary man, but one of early Pleistocene times.

¹ *Memoirs of Museum of Comparative Zoology, Harvard University*, vol. VI (1879).

² *Bulletin of Geological Society of America*, vol. II (1891), p. 189.

³ *Annual Report, Smithsonian Institution for 1899*, p. 419.

⁴ *University of California Publications in American Archaeology and Ethnology*, vol. VII (1908), p. 107.

⁵ *Ice Age in North America* (1911), p. 687.

It must be added that as accomplished a geologist as Clarence King extracted by his own hand a pestle out of the auriferous gravels beneath the basalt capping at Table Mountain, as is related by Becker. Sinclair's paper may be consulted for remarks on this discovery.

19. In 1889¹ W J McGee published a paper on his discovery of an obsidian implement in the wall of a canyon of Walker river, Nevada, at a point about fifteen miles from Lake Walker. The implement was found sticking point outwards in the old lake silt at a distance of 25 feet from the sununit. McGee examined carefully the condition surrounding the object and was unable to find any reason for believing that it had been introduced after the laying down of the silt. His discussion of the subject is commendable. In the same deposits of ancient Lake Lahontan he found bones of elephant, bison, horse, and camel. In I. C. Russell's work on Lake Lahontan² reference is made to McGee's discovery. The bones and implement were buried in the clays, which were regarded by McGee, G. K. Gilbert, and Russell as having been laid down during the waning of the later of the two glacial epochs which they recognized and as thus belonging to a time not far removed from the Recent.

On the other hand the presence of horse and camel remains must push the age of the clays back to the first third of the Pleistocene, apparently not later than the close of the Kansan glacial stage. In what are deposits of probably the same age laid down in old Lake Bonneville have been found remains assigned to *Bison latifrons*.³ The remains probably belonged to another species of *Bison*, but they indicate an age at least pre-Wisconsin.

It seems apropos to cite here another testimonial to the fact that man lived in the New World during early Pleistocene times. Some years ago, in a dry cave in southernmost Patagonia, there were found a part of a skin and many bones of the great ground-sloth *Grypotherium*, related to *Myodon*. There were found evidences that men had occupied the same cave and had possibly

¹ *American Anthropologist* (N. S.), vol. 2, pp. 301-312.

² *Monograph* XI, U. S. Geological Survey, pp. 247-269.

³ King, U. S. Geological Exploration of the 40th Parallel, vol. 1, p. 494.

domesticated the sloth. Associated with these remains of the sloth and of man were those of various extinct Pleistocene animals. Dr. A. S. Woodward concluded a paper¹ by saying that zoologists and geologists could not fail to agree that the ground sloth belonged to the Pampean fauna, and that they could hardly refuse to believe that the animal was actually kept and fed by an early race of man. The Pampean fauna belongs to the Pleistocene.

It seems to the writer that the cases above cited testify strongly to the fact that during early and middle Pleistocene times there existed in North America a population which was spread from the Atlantic Ocean to the Pacific; and that it left many tangible traces of its presence.

When Thomas Wilson, as above cited, called attention to the significance of the high degree of mineralization of the human bone found at Natchez, he made the judicious remark that one swallow does not make a summer. It may not be wrong to suggest, however, that when they come in flocks one has a right to conclude that at least the vernal season is on.

Doubtless in almost every case of discovery of an object buried in the earth some weak point can be found in the evidence; for the conditions are always complex. Most discoveries are made accidentally and by men who do not know the significance of the objects, nor what observations to make and record. One might wish that such men could have a presentiment of the discovery that was looming and could have at hand a geologist or a physical anthropologist to witness its advent; but it appears that expert geologists receive little more consideration than expert well-diggers. It seems to the writer that our friends the anthropologists give too little weight to the cumulative effects of the reported discoveries of Pleistocene man. That juror would render a bad verdict who would direct his attention to the weak points of the testimony of a lot of witnesses, to their stupidities, evasions and contradictions, and not regard the consistent story which ran through the testimony of all.

Among the most familiar objections offered against the authen-

¹ *Proceedings of Zoological Society of London* (1900), p. 78.

ticity of reported discoveries of early man are these: There was no scientific expert present; there were no witnesses to corroborate the statements of the finder; the object may have fallen down from some later deposit to the spot where picked up; it and the accompanying fossil bones may have been washed down quite recently from some older deposit; it may have worked its way down through a crevice or through some animal's burrow or by a hole left by a decayed root; some seeker after notoriety or an unscrupulous collector may have tried to impose on the public; or the ubiquitous joker may have exercised his wits on an unsophisticated geologist or anthropologist. Certainly all these are possible geological agencies and must be taken into account; but to what extent do they operate?

What is to be said about the geographical and stratigraphical distribution of alleged discoveries of early man in this country? A great sweep of territory extending from the Atlantic to the Pacific, including large portions of our most densely inhabited states is covered over with drift deposits belonging to four or five glacial stages. Into this drift materials there have been sunken innumerable cellars and dug wells, many shafts to reach coal and to prepare foundations for bridges and large buildings. In numbers unreckoned there have been wells bored to reach water and gas and petroleum. Railways have been cut through thousands of hills composed of glacial till. Along nearly every river and brook within the glaciated region are found banks, often many feet high, which offer fine sections of one or the other of the different glacial sheets. In this region gravity acts as elsewhere and so does the transporting power of water. The drift may be about as liable to crack and slump as is the loess. In this region the trees may or may not send down as deep their roots as in the loess of Missouri or the muck beds of Florida, but they do penetrate the drift. The gophers, prairie dogs, woodchucks, and badgers living on the drift are doubtless just as industrious as elsewhere. It is not to be doubted that every man who knows anything about geology, or paleontology, or has any inkling of anthropological questions has scanned eagerly the sections of the drift along our railroads and

our streams in the hope of finding there some relic of former man. Now, how many discoveries of such relics have been reported as having been made in the undisturbed drift of the ground moraines and terminal moraines?¹ With perhaps a single exception, the writer has been able to learn of none. The literature of the subject has been pretty well examined; and application to eminent glaciologists has been made and no cases have been discovered. The exception made is the discovery reported by G. F. Wright;¹ but here too the find was in gravel along a river, and this gravel was possibly reworked glacial materials. Even in a terminal moraine, where the glacial ice alternately advanced and receded, there might be found relics of some hardy race which lived close to the ice front; but men could not have lived under the glacier. It is for our friends the anthropologists to reflect why discoveries of human remains and artifacts are not reported from unmodified drift deposits. Why do the geological agencies which have been invoked so often in other regions not act also in the glacial till? It looks as if their value was reduced to almost negligible quantities. And one might further inquire whether, when they live and move on glacial drift, rascally collectors become more honest or more circumspect; jokers less facetious; and geologists and anthropologists less credulous?

Over large portions of our country south of the glaciated region there is spread out a mantle of materials which have resulted from the weathering of deposits of various ages. These constitute the greater part of Dr. W J McGee's Lafayette formation. They consist of gravels, sands, and clays and cover an area of more than 200,000 square miles; and they extend from near the coasts of the Atlantic Ocean and the Gulf of Mexico to elevations of from 500 to 1500 feet. The areas occupied by these deposits were formerly well populated by Indians; and the materials appear to be well adapted to receive artifacts through the accidental agencies which have been mentioned above. Moreover, in many places the materials have been considerably eroded, so that such inclusions might be brought to view. The writer has not been able to learn

¹ *Ice Age in North America*, pp. 651-653.

that any one has ever set up a claim to have discovered in this so-called Lafayette any traces of early man.

It appears to the writer that the matter may be summed up thus: In Pleistocene deposits laid down under conditions which permitted the existence of man we find numerous evidences of his presence. In Pleistocene deposits originating under conditions forbidding the presence of man and in older accumulations no traces of man's existence are met with, even though both those of Pleistocene age and the older ones are adapted to receive adventitious inclusions of artifacts.

The opposition that is presented against the idea that a race like our present Indians lived in our country during Pleistocene times is based on the views that European anthropology furnishes us a scale by means of which may be measured the time and the manner of development of the human race over the whole world. Now, it is not at all probable either that the human race had its origin in Europe or that the rest of the world received its populations from Europe. It is far more probable that man appeared first in Asia; perhaps, as Arldt¹ suggests, on the Thibetan plateau. From this center, the races streamed forth in all directions, somewhat as is shown on Arldt's plate 23. It is improbable that the Heidelberg and Neanderthal men had anything to do with the present human races, except in having had, somewhere back in the Tertiary, an origin in common. They represented species of beings different from *Homo sapiens*, species that attained a certain stage of bodily and mental development and then were exterminated by the superior races. If they too were of Asiatic origin, they were swept westward by the advancing waves of more intelligent tribes until reaching Europe they for a time held their pursuers at bay. Meanwhile the superior races were probably populating the other continents and the islands of the sea; and this occupation probably went on during late Pliocene and early Pleistocene times. We cannot avoid the conclusion that about the close of the Pliocene and probably at different times during the Pleistocene there was free communication between the lands of northeastern Asia and northwestern

¹ *Die Entwicklung der Kontinente*, p. 606.

America. Over the wide land-bridge existing there the animals of Asia swarmed into America and American animals into Asia. If man existed in Asia at that time, there appears to be no reason why he should not have accompanied on this journey the beasts with which he had associated.

It is to be noted at this point that there is by no means agreement in Europe among the anthropologists on the one hand and geologists on the other, regarding the time when the first men-like creatures reached Europe and regarding the glacial and the interglacial stages during which the various primitive races flourished in that country. The early cultures which by the anthropologists are put in the third interglacial are relegated by Penck, Geikie, and Wiegiers to the second interglacial stage; and by Wiegiers the pre-Chellean is put back into the first interglacial.

We have now to inquire whether or not a race of men with about the grade of development of our North American Indians would have been out of harmony with the rest of the mammalian fauna during the first interglacial stage in North America, that known as the Aftonian. The writer believes that the ancestor of the present races who should have existed during the first third of the Pleistocene in a stage represented by the Heidelberg man would have been an anomaly. It seems evident that not another land mammal belonging to the Quaternary epoch of North America or Europe has undergone anything like the profound changes which man would need to have undergone in order to make such ancestry possible.

At the outset, the collections which have been made in early Pleistocene deposits at various places in North America show from 20 to 50 per cent. or more of species which still exist, and which have undergone no changes that are perceptible. Among these may be mentioned the Virginia deer, the beaver, the brown bear, the raccoon, the glutton, the gray wolf, the coyote and the gray fox. Possibly with more complete materials some of these might be separated specifically. Other species are with difficulty distinguishable, or not at all, from existing ones, as one of the fossil tapirs. Many others are certainly specifically different, but the differences

are not great. There are several species of early Pleistocene horses; but when one has the skull and even the skeleton of one of these the characters distinguishing it from the existing horses are often not easy to discover; and one must often fear that he may have had palmed off on him the remains of the domestic horse. As regards the development of the brain, that of a horse from the Sheridan beds is as large as that of the domestic horse or even larger. The early Pleistocene bisons resembled greatly our existing bison, but had longer horns. The extinct tapirs of the early Pleistocene differed little from those now living; and the same is true of various feline and canine forms. The giant beaver, *Castoroides ohioensis*, appears to have lived from the beginning to the end of the Pleistocene. Just why it did not continue on to our day is not known. The same remark is true of the American mastodon, of *Elephas columbi*, and *E. primigenius*. Similar observations may be made regarding the stage of development of the early Pleistocene animals of Europe. Why should it be supposed that our ancestors were so retarded in their evolution until past the middle of the Pleistocene and that this evolution then should have suddenly been quickened? The various divisions of the Pleistocene differ from one another and from the Recent epoch principally in the successive extinction of animals, especially of the large and striking species; and not so much in changes in structure. Taking into consideration the rather unimportant morphological advancement made by the great majority of vertebrates since the early Pleistocene and the extinction of so many highly differentiated forms one might be pardoned for entertaining the view that on the whole the vertebrates have lost ground.

It may be permitted now to inquire whether the nearest relatives of man, the higher apes, have undergone important structural changes since the geological epoch when they first present themselves to us. In the Siwalik deposits of northern India, which belong to the latter part of the Miocene or early Pliocene, there has been found a palate of an ape which Lydekker referred to the genus *Troglodytes* (now *Pan*), the type of which is the existing chimpanzee. Lydekker stated that the teeth differed from those of the chim-

panzee in presenting a still more marked approach to the human type of dentition. In the same deposits was found a canine tooth of an ape which resembled so closely the corresponding tooth of the orang-utan that it was impossible to distinguish the two. There existed a species of *Semnopithecus* whose molars were extremely like those of the existing proboscis monkey. Two species of *Cynocephalus* were discovered, with regard to which Lydekker remarked that it could not be certainly affirmed that they were distinct from yet existing species of baboon. If these animals had at that early time reached such a high grade of development and if this has been maintained without important change to our day, why should it be thought a thing incredible that a race of men had attained in the first third of the Pleistocene the native American level and that it has not departed greatly from it up to the present?

The anthropologists who have discussed the discoveries made at Vero have impressed us with the fact that the human bones and the implements are identical with those of rather recent Florida Indians. They are the men to decide this question and the rest of us must submit. However, Dr. MacCurdy in his last article on the subject, indicates that this is not the whole of the matter. The bone points, he tells us, may be duplicated from southern and far southwestern mounds. The bone fish-hooks are exactly such as have been found in Connecticut. The same, he says, is true regarding the pottery. Might he not also have extended the range of similar objects both in space and time, in space to considerable parts of the Old World and in time well back into the Pleistocene of Europe? Is it not true likewise that, while the human bones resemble those of recent Florida Indians, they resemble also those of most other Indians? Under the circumstances, it does not appear to be necessary to conclude that these bones and artifacts were derived from Florida Indians living within 4000 years.

If the reader will consult a paper written by H. Klaatsch¹ he will find that in Tasmania and Australia the majority of the artifacts were similar to those of Mousterian times in Europe. In Australia were found chipped implements which were quite like

¹ *Zeitschrift für Ethnologie*, vol. 40 (1908), pp. 407-428.

those referred in Europe to the Chellean and Acheulean; others were even more primitive than the supposed eoliths of the Miocene and Oligocene. It is evident, therefore, that some races of men stand still in their culture or move so slowly that their progress is imperceptible.

Finally, Dr. MacCurdy offers as evidence against the great age of the pottery found at Vero the fact that not even the rudest pottery is found in the argillite culture in the Delaware valley. This argument loses its force when it is considered that even within historical times many Indian tribes have had no knowledge of pottery.

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THE GROWTH OF BOYS: DENTITION AND STATURE

BY LESLIE SPIER

STUDIES of growth usually give measurements of individuals based on their actual or "chronological" ages and not on their physiological status. The following relations of stature to status as defined by dentition are based on plaster casts of the dental arches and measurements of some three hundred and fifty school-boys of Utuado, Porto Rico, collected by Professor Franz Boas in 1915. The method of treatment was suggested by Professor Boas.

It has been shown¹ that the observed characteristics of curves of growth may be explained as due to variations in the period of development, *i. e.*, to accelerations and retardations. Since the stage of development of a child, at any period, depends on the previous acceleration or retardation of its growth, which affects the body as a whole, it follows that all measurements of the child must vary together; either all values must be increased beyond the average of its age by acceleration or all depressed by retardation. The more rapid the rate of growth, the greater is the effect of variation in period on all the measurements, that is, correlation between all measurements increases during the period of rapid growth and declines during the period of decreasing growth. The variation in period as expressed by a body measurement must be the variation of period at which a particular physiological status is attained. We find, therefore, a correlation between observed physiological status and body measurements. Such a correlation of pubescence to stature, weight, strength of the right hand and to mental development as indicated by school standing has been shown by Crampton,² to head length and width and to width of

¹ Boas, Franz, and Wissler, Clark, "Statistics of Growth," *Report of the Commissioner of Education for 1904* (Washington, 1905), pp. 25-132.

² Crampton, C. Ward, "Physiological Age—A Fundamental Principle," *American Physical Education Review*, vol. XIII (Springfield, Mass., 1908), pp. 141, 214, 268, 345.

face by Boas,¹ and Deaver's data indicates the same relation of pubescence to strength of the forearms and shoulder retractors and to lung capacity.² We should expect a similar correlation of physiological development as indicated by dentition with stature.

Crampton has demonstrated a correlation of dental development with stature and weight. Among 934 New York boys, at a certain age individuals were taller and heavier in proportion to the number of permanent canines and second molars they had erupted.³ In this study the eruption of corresponding upper and lower teeth is considered as a single phenomenon. However, there is usually an interval of about a year between their eruption; therefore, these results are somewhat invalidated. While the present series is smaller, about 350 individuals, the study has been extended to all the deciduous and permanent teeth.

Physiological periods for the Porto Rican boys may be obtained from the data on dentition. The following tables show the number

TABLE I
NUMBER OF DECIDUOUS TEETH PRESENT

Age	Cases	Upper Jaw					Lower Jaw		
		Inner Incisors	Outer Incisors	Canines	First Molars	Second Molars	Canines	First Molars	Second Molars
7	25	7	18	24	22	23	20	19	19
8	44	7	16	31	33	36	24	30	29
9	38	..	3	23	17	24	12	15	17
10	74	..	10	31	19	47	18	28	30
11	65	..	2	16	11	27	9	7	21
12	56	7	3	10	2	4	10
13	81	..	2	8	3	9	2	4	11
14	84	..	1	1	1
15	60

of deciduous and permanent teeth present among a total of all possible cases of teeth of each kind; teeth, not individuals, are the units. Permanent teeth protruding beyond the gums to any degree

¹ Boas, Franz., "Changes in Bodily Form of Descendants of Immigrants," *Reports of the Immigration Commission* (Washington, 1911), pp. 38, 129-137.

² Deaver, G. Gilbert, "Strength and Lung Capacity in Relation to Pubescence," MS. International Young Men's Christian Association College, Springfield, Mass., 1912.

³ Crampton, pp. 351-355.

are counted as erupted teeth. Any recognizable fragment of a deciduous tooth was counted. Recognition is difficult in plaster casts and mechanical causes operate more freely on the fragments than on permanent teeth, hence, the tables for deciduous teeth are not so smooth as for the permanent.

TABLE II
PERCENTAGES OF DECIDUOUS TEETH PRESENT

Age	Upper Jaw					Lower Jaw		
	Inner Incisors	Outer Incisors	Canines	First Molars	Second Molars	Canines	First Molars	Second Molars
7	29	70	96	89	93	80	77	77
8	16	37	70	76	82	55	67	65
9	..	9	61	46	63	32	40	45
10	..	14	42	26	63	24	38	41
11	..	3	24	17	42	14	11	33
12	13	5	18	4	7	18
13	..	2	10	4	11	2	5	13
14	..	1	1	1
15

TABLE III
NUMBER OF PERMANENT TEETH PRESENT

Age	Cases	Upper Jaw							Lower Jaw				
		Inner Incisors	Outer Incisors	Canines	First Bicus-pids	Second Bicus-pids	First Molars	Second Molars	Canines	First Bicus-pids	Second Bicus-pids	First Molars	Second Molars
7	23	9	3	..	1	..	21	..	4	19	..
8	44	37	15	9	12	4	44	..	16	5	1	43	..
9	38	38	31	11	21	10	38	3	19	15	8	36	5
10	71	71	57	29	46	23	71	9	50	35	21	70	12
11	65	65	62	40	54	36	65	23	49	51	38	65	31
12	54	54	54	42	51	43	54	34	48	44	38	54	42
13	80	80	80	71	76	70	80	64	78	76	67	80	63
14	84	84	84	81	84	83	84	80	83	83	82	84	80
15	56	56	56	56	56	56	56	53	56	56	52	56	50
16	33	33	33	33	33	32	33	33	33	33	33	33	31
17	23	23	23	23	23	23	23	22	23	23	23	23	23
18	21	21	21	21	21	21	21	20	21	21	21	21	21
19	11	11	11	11	11	11	11	11	11	11	11	11	11

These give, for the loss of the deciduous teeth and the eruption of the permanent teeth, the following average ages and variabilities:

TABLE IV
PERCENTAGES OF PERMANENT TEETH PRESENT

Age	Upper Jaw							Lower Jaw				
	Inner Incisors	Outer Incisors	Canines	First Bicus-pids	Second Bicus-pids	First Molars	Second Molars	Canines	First Bicus-pids	Second Bicus-pids	First Molars	Second Molars
7	46	13	..	4	..	93	..	16	84	..
8	84	33	20	27	8	100	..	36	11	2	98	..
9	100	82	30	54	28	..	9	50	40	20	95	12
10	..	80	41	65	32	..	12	70	49	29	99	17
11	..	96	62	83	56	..	35	75	79	58	100	47
12	..	100	77	94	79	..	63	88	81	70	..	77
13	89	95	87	..	80	98	95	84	..	79
14	97	100	99	..	95	99	99	98	..	95
15	100	..	100	..	95	100	100	93	..	90
16	97	..	100	100	..	94
17	100	..	97	100
18	96
19	100

TABLE V
LOSS OF THE DECIDUOUS TEETH¹

	Outer Incisors	Canines	First Molars	Second Molars
Upper.....	7.8 ± 1.6	9.5 ± 2.5	9.1 ± 1.9	10.3 ± 2.1
Lower.....	8.4 ± 2.2	8.9 ± 2.2	9.2 ± 3.2

TABLE VI
ERUPTION OF THE PERMANENT TEETH

	Inner Incisors	Outer Incisors	Canines	First Bicus-pids	Second Bicus-pids	Second Molars
Upper.....	8.5 ± 1.4	10.3 ± 2.2	9.2 ± 2.0 ²	10.7 ± 2.0 ²	12.1 ± 1.7
Lower.....	6.2 ± 1.5	7.0 ± 1.3	9.0 ± 2.3	9.9 ± 1.8	10.9 ± 2.0	11.4 ± 2.0

¹ Anterior-posterior lengths of permanent and deciduous upper teeth were incidentally obtained:

Permanent inner incisor.....	8.6 ± 0.7 mm.	618 cases.
“ outer “	6.5 ± 0.7	547
“ first bicus-pids.....	6.7 ± 0.5	158
“ second “	6.4 ± 0.5	161
Deciduous inner incisor.....	6.1 ± 0.4	16
“ outer “	5.1 ± 0.7	50

² Upper bicus-pids appear earlier than the corresponding teeth of the lower jaw in a mixed series of 4,850 boys and girls collected by James and Pitts, *Proceedings Royal Society of Medicine*, vol. 5 (London, 1912), pp. 80-101, and in a similar series of about 4,000 boys and girls by Livy, *British Medical Journal*, vol. 11 (London, 1885), p. 241.

For comparison, we have average ages and variabilities for the eruption of permanent teeth of the upper jaw based on plaster casts collected by Dr. Walter Channing (probably of Boston school children). These averages¹ are based on the number of children having permanent teeth among the total number of children, *e. g.*, a child with one or both permanent canines was counted as a child with permanent canines. Individuals, not teeth, are the units.² The averages are not strictly comparable, since by this method slightly earlier average ages are obtained than by using the teeth as units. We have also separated first and second bicuspid on the same casts.

TABLE VII
NUMBER OF UPPER BICUSPIDS PRESENT

Age	Boys			Girls		
	Cases	First Bicuspid	Second Bicuspid	Cases	First Bicuspid	Second Bicuspid
6	46	1	..	92	4	2
7	60	1	..	76	12	3
8	56	12	6	60	19	9
9	84	33	16	82	41	21
10	62	39	20	65	41	22
11	46	35	25	56	36	29
12	40	32	22	44	36	26
13	24	21	16	34	32	25
14	26	25	20	20	18	16
15	14	14	14

TABLE VIII
PERCENTAGES OF UPPER BICUSPIDS PRESENT

Age	Boys		Girls	
	First Bicuspid	Second Bicuspid	First Bicuspid	Second Bicuspid
6	2	..	4	2
7	2	..	16	4
8	21	11	32	15
9	39	19	50	26
10	63	32	63	34
11	76	54	64	52
12	80	55	82	59
13	87	67	94	74
14	96	77	90	80
15	100	100

¹ *Statistics of Growth*, p. 34.

² Information from Dr. Clark Wissler.

The average ages and variabilities for the Channing Series are as follows:

TABLE IX

	Inner Incisors	Outer Incisors	Canines	First Bicuspid	Second Bicuspid	Second Molars
Boys.....	7.5 \pm 1.4	9.5 \pm 2.1	11.2 \pm 1.4	9.4 \pm 2.2	11.4 \pm 2.9	13.2 \pm 2.0
Girls.....	7.0 \pm 1.6	8.9 \pm 2.1	11.3 \pm 1.0	9.5 \pm 2.5	11.2 \pm 2.9	12.8 \pm 1.6

As these figures stand, the Porto Rican boys appear to erupt their teeth as much as a year in advance of the Boston boys. However, the meaning of this difference is obscure, since Channing and Wissler found that a group of feeble-minded children erupted their canines and second molars a corresponding period in advance of the same Boston boys.¹

TABLE X

	Boys	Girls
Canines.....	10.7 \pm 1.4	10.0 \pm 1.7
Second molars.....	10.9 \pm 1.9	11.8 \pm 0.9

If there is a correlation between physiological status and body measurements, then, at a certain age, those individuals accelerated beyond the average dental stage of their age should show a greater average stature than the remaining individuals. For example, eight-year-old boys with permanent upper canines are more developed than their age-mates who lack these teeth; as accelerated individuals, the group with canines should have an average stature greater than that of the group without canines. Similarly, a group lacking deciduous canines should be taller than a group of the same age with these teeth.

In tables XI and XII are given the average statures and variabilities of boys separated on the basis of the presence and absence of the deciduous teeth and the absence and presence of the permanent teeth, tooth by tooth.

¹ Channing, Walter, and Wissler, Clark: "The Hard Palate in Normal and Feeble-Minded Individuals," *Anthropological Papers, American Museum of Natural History*, vol. 1 (New York, 1908), pp. 316-319.

TABLE XI

STATURE OF BOYS WITH DECIDUOUS TEETH PRESENT AND ABSENT
(Statures in millimeters)

Upper Jaw

Age	Outer Incisors		Canines		First Molars		Second Molars	
	Present	Absent	Present	Absent	Present	Absent	Present	Absent
7	1141 ± 48	1139 ± 69
8	1166 ± 35	1189 ± 61	1182 ± 53	1173 ± 57	1177 ± 41	1187 ± 85	1181 ± 49	1170 ± 74
9	1245 ± 40	1268 ± 48	1243 ± 39	1263 ± 47	1241 ± 44	1276 ± 36
10	1266 ± 103	1276 ± 60	1242 ± 59	1299 ± 65	1247 ± 40	1284 ± 72	1274 ± 63	1275 ± 75
11	1263 ± 47	1325 ± 53	1277 ± 74	1317 ± 48	1296 ± 65	1321 ± 50
12	1336 ± 66	1389 ± 75	1356 ± 86	1386 ± 72
13	1308 ± 84	1394 ± 77	1294 ± 109	1397 ± 71

Lower Jaw

8	1195 ± 57	1155 ± 44	1195 ± 57	1155 ± 44	1182 ± 56	1168 ± 48	1178 ± 58	1176 ± 47
9	1241 ± 48	1260 ± 48	1241 ± 48	1260 ± 48	1241 ± 32	1261 ± 53	1252 ± 44	1253 ± 49
10	1253 ± 50	1281 ± 72	1242 ± 54	1294 ± 69	1260 ± 71	1284 ± 62
11	1235 ± 43	1317 ± 51	1265 ± 75	1311 ± 53	1281 ± 61	1319 ± 50
12	1383 ± 83	1382 ± 78
13	1307 ± 102	1398 ± 270

TABLE XII
STATURE OF BOYS WITH PERMANENT TEETH ABSENT AND PRESENT
Upper Jaw

Age	Outer Incisors		Canines		First Bicuspids		Second Bicuspids		Second Molars	
	Absent	Present	Absent	Present	Absent	Present	Absent	Present	Absent	Present
8	1165 ± 41	1213 ± 64	1188 ± 52	1145 ± 49	1176 ± 41	1186 ± 81	1181 ± 45	1157 ± 95
9	1249 ± 23	1254 ± 48	1257 ± 42	1260 ± 47	1243 ± 39	1263 ± 47	1242 ± 41	1284 ± 36
10	1264 ± 78	1279 ± 63	1251 ± 58	1308 ± 68	1248 ± 57	1289 ± 69	1270 ± 48	1285 ± 71	1269 ± 61	1342 ± 69
11	1284 ± 51	1326 ± 57	1280 ± 80	1317 ± 51	1297 ± 65	1320 ± 51	1305 ± 60	1321 ± 53
12	1341 ± 60	1409 ± 64	1356 ± 86	1386 ± 72	1352 ± 62	1397 ± 77
13	1317 ± 82	1394 ± 77	1334 ± 134	1395 ± 67	1338 ± 103	1400 ± 68

Lower Jaw

Age	Outer Incisors		Canines		First Bicuspids		Second Bicuspids		Second Molars	
	Absent	Present	Absent	Present	Absent	Present	Absent	Present	Absent	Present
7	1109 ± 26	1174 ± 60
8	1173 ± 40	1179 ± 57	1187 ± 57	1159 ± 48
9	1245 ± 50	1264 ± 45	1238 ± 36	1267 ± 51	1249 ± 41	1265 ± 50
10	1248 ± 48	1285 ± 73	1249 ± 57	1293 ± 70	1262 ± 62	1305 ± 76	1266 ± 56	1335 ± 70
11	1266 ± 55	1317 ± 52	1292 ± 65	1310 ± 54	1287 ± 61	1321 ± 49	1292 ± 58	1323 ± 51
12	1302 ± 74	1385 ± 79	1353 ± 81	1387 ± 78	1352 ± 73	1392 ± 78	1335 ± 76	1394 ± 75
13	1314 ± 96	1400 ± 70	1343 ± 79	1396 ± 80

TABLE XIII
STATURE OF BOYS ON THE BASIS OF ALL TEETH

Age	Upper Jaw				Lower Jaw				Upper and Lower Jaws					
	Permanent		Deciduous		Permanent and De- ciduous		Deciduous		Permanent and De- ciduous		Permanent and De- ciduous			
	Absent	Present	Present	Absent	Permanent		Present	Absent	Permanent and De- ciduous		P. Absent D. Present	P. Present D. Absent		
					P. Absent D. Present	P. Present D. Absent			P. Absent D. Present	P. Present D. Absent				
7	1142 ± 51	1147 ± 61	1146 ± 52	1134 ± 56	1144 ± 51	1141 ± 59	1138 ± 52	1154 ± 55	1144 ± 55	1143 ± 54	1140 ± 53	1149 ± 54	1142 ± 52	1146 ± 56
8	1178 ± 47	1181 ± 60	1177 ± 46	1182 ± 61	1177 ± 46	1182 ± 60	1181 ± 51	1174 ± 54	1182 ± 55	1175 ± 51	1181 ± 52	1175 ± 53	1179 ± 49	1178 ± 56
9	1249 ± 40	1259 ± 46	1243 ± 40	1260 ± 45	1247 ± 40	1260 ± 45	1248 ± 42	1256 ± 45	1249 ± 45	1258 ± 46	1249 ± 42	1257 ± 45	1247 ± 41	1259 ± 45
10	1202 ± 63	1283 ± 68	1259 ± 64	1280 ± 68	1261 ± 63	1282 ± 68	1259 ± 57	1282 ± 69	1252 ± 60	1280 ± 68	1257 ± 58	1281 ± 69	1259 ± 61	1282 ± 68
11	1204 ± 61	1316 ± 55	1281 ± 63	1317 ± 54	1290 ± 62	1316 ± 54	1286 ± 59	1313 ± 55	1267 ± 61	1313 ± 54	1281 ± 60	1313 ± 54	1286 ± 61	1315 ± 54
12	1353 ± 69	1387 ± 73	1357 ± 71	1383 ± 73	1354 ± 69	1385 ± 73	1349 ± 73	1385 ± 75	1319 ± 74	1382 ± 74	1341 ± 74	1384 ± 74	1348 ± 71	1384 ± 74
13	1324 ± 105	1391 ± 76	1308 ± 93	1396 ± 75	1319 ± 100	1393 ± 76	1314 ± 88	1390 ± 78	1351 ± 45	1399 ± 74	1292 ± 84	1397 ± 76	1303 ± 90	1394 ± 76

These data, together with a few cases referring to teeth not tabulated, are averaged below for corresponding values for the upper and lower jaws separately and finally together (table XIII). This is in effect simply weighting the stature of each individual according to his dental pattern.

After the ninth year the average statures of the two groups differ, the absolute difference increasing to the fourteenth year (see particularly the last two columns). These values show that there is a functional relation between stature and physiological status as defined by dentition.

For comparison, the upper bicuspid of boys and girls of the Channing series have been treated in the same manner with regard to stature and weight (tables XIV and XV).

TABLE XIV
STATURE OF CHILDREN WITH UPPER BICUSPIDS ABSENT AND PRESENT

Age	Boys				Girls			
	First Bicuspid		Second Bicuspid		First Bicuspid		Second Bicuspid	
	Absent	Present	Absent	Present	Absent	Present	Absent	Present
7	1162 ± 44	1200 ± 46
8	1229 ± 47	1244 ± 73	1232 ± 53	1231 ± 60	1216 ± 44	1216 ± 43	1217 ± 44	1214 ± 44
9	1267 ± 59	1282 ± 54	1297 ± 66	1286 ± 47	1250 ± 55	1292 ± 84	1264 ± 61	1292 ± 107
10	1334 ± 68	1319 ± 62	1310 ± 59	1339 ± 65	1296 ± 67	1318 ± 64	1311 ± 68	1312 ± 63
11	1373 ± 73	1319 ± 91	1348 ± 69	1319 ± 103	1323 ± 68	1351 ± 75	1329 ± 80	1351 ± 67
12	1374 ± 74	1399 ± 84	1381 ± 83	1405 ± 86	1335 ± 56	1423 ± 73	1374 ± 80	1430 ± 67
13	1438 ± 122	1433 ± 89	1448 ± 118	1527 ± 56

This smaller series gives affirmative results for both statures and weights.

TABLE XV
WEIGHT OF CHILDREN WITH UPPER BICUSPIDS ABSENT AND PRESENT
(Weight in Pounds Avoirdupois)

Age	Boys				Girls			
	First Bicuspid		Second Bicuspid		First Bicuspid		Second Bicuspid	
	Absent	Present	Absent	Present	Absent	Present	Absent	Present
7	54.7 ± 7.1	58.2 ± 6.0
8	54.8 ± 7.4	58.7 ± 5.2	55.3 ± 7.2	57.8 ± 6.2	56.0 ± 7.7	54.4 ± 5.2	56.1 ± 7.1	52.3 ± 4.6
9	60.0 ± 9.6	58.8 ± 7.6	57.7 ± 10.3	60.8 ± 8.2	54.9 ± 5.9	60.2 ± 10.0	56.1 ± 7.2	61.5 ± 11.1
10	61.9 ± 6.9	62.6 ± 7.5	60.8 ± 6.4	65.7 ± 7.9	62.0 ± 8.8	63.2 ± 8.9	62.9 ± 9.1	61.4 ± 8.5
11	68.8 ± 14.3	68.6 ± 7.3	60.2 ± 11.3	70.8 ± 6.6	65.0 ± 8.1	64.7 ± 7.7	64.6 ± 7.9	65.1 ± 8.1
12	66.4 ± 11.7	71.5 ± 12.4	67.7 ± 3.0	73.2 ± 12.3	65.2 ± 10.9	77.3 ± 12.7	73.2 ± 16.1	76.0 ± 10.8
13	76.3 ± 9.1	79.1 ± 18.4	89.3 ± 22.3	89.5 ± 20.8

Having shown that variations in stature correspond to variations in the period of physiological development, we can obtain an insight into the character of the variations in period by considering each measurement as corresponding to the age to which it would belong according to the table of average statures for all boys (table XVI).

TABLE XVI
STATURES AND WEIGHTS OF ALL BOYS AND GIRLS

Age	Porto Rican Boys		Channing Series					
			Boys			Girls		
	Cases	Stature	Cases	Stature	Weight	Cases	Stature	Weight
6	23	1131±38	52.1± 8.5	46	1126±43	51.3± 6.5
7	14	1144± 56	30	1165±60	53.6± 7.5	39	1168±45	55.3± 7.0
8	26	1179± 53	27	1232±53	55.6± 7.0	31	1216±43	55.5± 7.0
9	23	1254± 45	40	1273±57	59.5± 8.7	42	1271±73	57.5± 8.6
10	39	1275± 68	33	1323±63	62.3± 7.3	34	1310±65	62.8± 8.8
11	37	1311± 57	21	1332±89	68.7± 9.2	30	1340±73	64.9± 7.9
12	30	1381± 75	21	1394±81	70.7±12.3	22	1407±77	75.1±13.0
13	42	1386± 82	12	1434±98	78.2±15.8	16	1506±97	89.5±20.8
14	47	1468± 90	12	1552±85	88.1± 3.0	9	1541±99	99.9±30.7
15	32	1519±102	7	1599±53	108.0± 4.1
16	18	1571± 66
17	14	1602± 62
18	12	1627± 87
19	6	1620± 45

Confining our attention to the final values for Porto Rican boys (last two columns of table XIII), we can interpolate in this table for example for 1303 for stature and find 10.8 as the age for the corresponding average stature.

TABLE XVII

Age	Permanent Absent	Permanent Present
	Deciduous Present	Deciduous Absent
7.....	7.0	7.0
8.....	8.0	8.0
9.....	8.9	9.3
10.....	9.3	10.2
11.....	10.3	11.2
12.....	11.5	12.7
13.....	10.8	13.1

These ages obtained by dentition and stature probably differ more from the corresponding chronological ages than would ages obtained on the basis of stature alone.

At the beginning of the period of rapid increase in the rate of growth, the accelerated individuals must be growing at a higher rate than the retarded individuals of the same chronological age but who have not yet entered into the period of rapid growth. Conversely, at the end of the period of rapid growth, the accelerated individuals must grow at a slower rate than the retarded. Consequently, at the beginning of the period, accelerated individuals must vary more than retarded individuals from the average of their age and less at the end of the same period. We have compared below the variabilities of the retarded and accelerated groups of each age.

TABLE XVIII

Age	Retarded Group	Accelerated Group	Difference
7	52	56	+ 4
8	49	56	+ 7
9	41	45	+ 4
10	61	68	+ 7
11	61	54	- 7
12	71	74	+ 3
13	90	76	-14

While the results are not marked, the accelerated group varies more than the retarded group at the beginning of the period of rapid increase of the rate of growth, but less at the end of the same period. The converse holds true for the retarded group. From another point of view the increased variabilities in stature for both accelerated and retarded groups may be an expression of a different rate of development from that of dentition. If this is so, the greater the degree of acceleration or retardation of dental development, the greater will be the variations in stature.¹

These data show that there is a marked functional relation between stature and stage of dental development. Variations in stature, aside from such as are due to heredity and perhaps to other causes, are then expressions of variations in the period at which certain stages of development are reached.

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¹ Boas, Franz, "The Growth of Children," *Science*, N. S., vol. XXXVI, no. 937, p. 817.

ON COMPUTATIONS FOR THE MAYA CALENDAR

By RAYMOND K. MORLEY

IN *A Method which may have been used by the Mayas in Calculating Time*, and also in *The Numeration, Calendar Systems, and Astronomical Knowledge of the Mayas*, Appendix VII, Mr. C. P. Bowditch gave a rule for shortening the computations for finding a terminal date in the Maya calendar count when the initial date and the interval of time elapsed from it are given. This rule has perhaps not had the use it deserves, partly because of the natural prejudice of modern scholars in favor of expressing numbers in the familiar decimal system, and partly because the rule as given by Mr. Bowditch is still a little cumbrous. It is the purpose of the present paper, first, to develop modifications of Mr. Bowditch's method which make it more easily used, and second, to give a new formula for the solution of the reverse problem, namely, given two dates to find the interval of time between them. These deductions will be prefaced by some general considerations concerning computations in the Maya system of numeration. Their system, so far as we know it, was a curious mixture. The bar and dot notation is quinary, the face numerals from 13 (or sometimes 14) to 19 and the corresponding native names for these numbers are decimal, and the calendar system as a whole is vigesimal, with the exception of the step from uinals to tuns. Irregular though the system is, it is the author's belief that the greatest facility in handling Maya time periods will come by keeping them expressed so far as possible in the Maya fashion.

Some operations present no difficulty in an irregular system like the Maya. We handle repeatedly somewhat similar notations. For instance 4 hours 3 minutes 6 seconds; £12 13 s. 6 d.; $78^{\circ} 24' 30''$ and so on. There is no difficulty in performing simple operations on these. Adding them, subtracting them, multiplying or dividing them by small numbers are all quite easy, the only pre-

caution to be observed is the reduction of one kind of unit to the next in carrying or borrowing. So with the Maya system of time periods. For instance:

$$\begin{array}{r}
 17. 7. 6.12. 1 \\
 + \quad 4.15. 7.10.13 \\
 \hline
 1. 2. 2.14. 4.14
 \end{array}
 \qquad
 \begin{array}{r}
 3. 1.14. 6. 8 \\
 - \quad 1. 2. 6.17. 9 \\
 \hline
 1.19. 7. 6.19
 \end{array}$$

The only point requiring special care here is to note that the amount carried from uinals to tuns or borrowed from tuns to uinals is 18. Again as an example of multiplication by a small number let us find 11 times 2.12.13. 0.

$$\begin{array}{r}
 2.12.13.0 \\
 \times 11 \\
 \hline
 1. 8.19.17.0
 \end{array}$$

The steps are: $11 \times 0 = 0$. $11 \times 13 = 143$ which is (dividing by 18) 7 tuns and 17 uinals. Write the 17 uinals and carry 7 tuns. $11 \times 12 = 132$ and 7 to carry is 139 which is (dividing by 20) 19 and 6 to carry. $11 \times 2 = 22$ and the 6 makes 28, which is 8 and 1 to carry. Write the 1. Short division goes in a very similar way: to divide 14. 4.17. 3. 2. by 5:

$$\begin{array}{r}
 5) 14. 4.17. 3. 2 \\
 \hline
 2.16.19. 7.16 \text{ and a remainder of } 2.
 \end{array}$$

The steps are: 5 into 14 twice and 4 over; the 4 over is 4 twenties or 80 of the next grade, a total of 84 of the next grade; 5 into 84 16 times and 4 over, which makes a total of 97 of the next grade; 5 into 97 19 times and 2 over. 2×18 is 36 of the next grade and 3 makes a total of 39. 5 into 39 7 times and 4 over, 82 of the next grade. 5 into 82 16 times and a remainder of 2.

However, in the case of a multiplication by a larger number, so that handling it would require the use of partial products, our ordinary process breaks down. Suppose we try to multiply in the usual way, by means of partial products, 2.12.13.0 by 73. We can multiply by the 3 at once

$$\begin{array}{r}
 2.12.13.0 \\
 \times 3 \\
 \hline
 7.18. 3.0
 \end{array}$$

but the next partial product cannot be formed by multiplying by 7 and setting over one place, because the actual step is multiplying by 70 and the setting over in the Maya system is not equivalent to multiplying by 10 but by 20 in most cases, but to multiplying by 18 when setting across the 18 step from uinals to tuns. If it were not for this step, that is, if the Mayas used a purely vigesimal system, writing the multiplier also in the same system would solve the difficulty and allow us to apply our ordinary processes at once. A long division involves fully as many difficulties for a similar reason. This does not mean that these processes could not be carried out by some means, but that it is impractical because our usual rules do not apply to the operations. Fortunately, however, the calculations ordinarily required for the Maya calendar are of a very restricted variety, and by the use of special methods for these problems nearly all time units may be kept in the Maya system, avoiding the tedious and rather useless reduction to the decimal notation that has proven the popular method with most investigators (*e. g.*, Förstemann, Seler, S. G. Morley).

There are two principal problems that we have to solve in connection with the calendar: (1) Given a starting date by day coefficient, day name, month coefficient, and month name, and a period of time counted from it, required the terminal date by coefficients and names. (2) Given the two dates, required the distance from one to the other. Let us take them up in order.

As is explained in treatises on the Maya calendar finding one date a certain distance from another is unaffected by diminishing the distance by any number of complete calendar rounds of 52 haab or years. To subtract the greatest possible number of complete rounds diminishes the subsequent work somewhat.¹ 1 haab = 1.0.5. Multiply this by 52 as follows: 52×5 kins = 260 kins or 13 uinals; 52×1 tun = 52 tuns = 2 katuns and 12 tuns. Hence $1.0.5 \times 52 = 2.12.13.0$. That is 1 calendar round = 2.12.13.0. For short intervals, say less than 10 rounds, it is easy to multiply this at once for the purpose of getting the multiple to

¹ This process of diminishing the distance by calendar rounds, while the usual method, can be avoided, as will be shown later.

be subtracted. *E. g.*, 7 rounds = $7 \times 2.12.13.0 = 18.9.1.0$. The process is: $7 \times 0 = 0$, $7 \times 13 = 91 = 18 \times 5 + 1$, write 1 and carry 5; $7 \times 9 = 84$, and 5 to carry is 89 which = $4 \times 20 + 9$, write 9 and carry 4; $7 \times 2 = 14$, add 4 = 18. But for long intervals such as are found in the Initial Series this is too difficult. A table is therefore desirable. Such a table may be found in Morley, *Bureau of American Ethnology*, Bulletin 57, p. 144. The column headed Cycles, etc., is the one to use. When all complete calendar rounds have been subtracted the distance number to be treated is always less than 2.12.13.0.

The problem of finding the terminal date consists of three parts: (1) Counting from the given day name a number equal to the remainder on dividing the distance by 20 to find the new day name; (2) Counting from the day coefficient a number equal to the remainder on dividing the distance by 13; (3) Counting from the day in the year a number equal to the remainder on dividing the distance by 365 days or 1.0.5.

1. There is no difficulty with the first of these. The required number is exactly the kin number of the given distance, since 20 kins = 1 uinal, and the tuns, etc., are all exact multiples of the uinal. To get the day name count forward (backward sometimes when the count as a whole is backward. I shall hereafter assume that it is forward. The methods for finding the remainders apply) a number equal to the kin number.

2. The remainder on dividing by 13 might be found by the process indicated above for short division, but the following modification of the method of Mr. Bowditch already referred to is practically much more convenient. To understand it we shall first develop some general principles.

A. The remainder on dividing a number by a divisor is not affected by adding to or subtracting from the number any exact multiple of the divisor. For, regard the process of dividing as counting around and around in a circle composed of as many places as there are units in the divisor (of course it is from just such circular counting in the Maya calendar that the need for the remainder arises). Then it is clear that to add or subtract an

exact multiple of the divisor will change the number of complete rounds but will not affect at all the amount left over. If a number be diminished by the next less exact multiple of the divisor, that is if the number be replaced by the remainder on dividing by the divisor, the process is frequently referred to as "casting out" the divisor. Thus, casting out 13 from 68 gives 3; casting out 13 from 97 gives 6, etc.

B. If a number divided by a divisor gives a certain remainder, a multiple n times the number will have a remainder n times as great as the original remainder, or will differ from it by a multiple of the divisor. Thus, $12/9$ gives a remainder 3, hence 2×12 divided by 9 gives a remainder $2 \times 3 = 6$. Again, 5×12 divided by 9 gives a remainder 6 which $= 5 \times 3 - 9$. That this must be the case will appear from considering again the counting around a circle. For if the number once counted around and around ends a certain distance (*i. e.*, the remainder) beyond the starting point, counted twice it will end as much further beyond the new starting point, *i. e.*, twice as far from the first starting point, and so on. If these together make up more than a complete revolution it will be necessary to deduct a multiple of the divisor. Further examples follow: $20/13$ leaves a remainder of 7; therefore 4×20 , that is 80, leaves a remainder of $7 \times 4 - 2 \times 13 = 2$ as may readily be verified. $360 = 20 \times 18$. Hence $360/13$ gives remainder of $7 \times 18 = 126$ less 117 (13×9) or 9. This last is more easily done by the aid of Principle *A*, thus: $20 \times 18 = 20 \times 13 + 20 \times 5$. Casting out 20×13 (Prin. *A*) we may treat 20×5 instead. $5 \times 7 = 35$; casting out 13, $35 - 26 = 9$ as before. In using Principle *B*, therefore it is never necessary to use a multiplier larger than the divisor, in these cases 13. If one such occurs the remainder is unaltered by casting out 13 from it at once.

We will now apply these principles to finding the remainder on dividing 1 uinal, 1 tun, 1 katun, and 1 cycle (the cycle is unnecessary if all complete calendar rounds have been subtracted) respectively by 13.

1 uinal divided by 13 remainder 7.

1 tun = 18 uinals which will have same remainder as $18 - 13 = 5$ then $7 \times 5 = 35$ cast out 13 remainder 9.

1 katun = 20 tuns, same remainder as 7 tuns; then $7 \times 9 = 63$ cast out 13 remainder 11.

1 cycle = 20 katuns, same remainder as 7 katuns, then $7 \times 11 = 77$ cast out 13 remainder 12.

To put these results together we need another principle, *C*. Namely that the sum of the remainders of several numbers each divided by a divisor, after casting out the divisor from this sum, is the same as the remainder on dividing the sum of the numbers themselves.

Another consideration of counting in a circle will show the truth of this.

If then we have a number expressed in cycles, katuns, uinals, and kins the total remainder is given by the following sum

kin number + uinal number $\times 7$ (7 is remainder on dividing 1 uinal by 13, then apply Principle *B*),
 + tun number $\times 9$ (9 is remainder from 1 tun as found on p. 53, then by Principle *B*),
 + katun number $\times 11$ (11 is remainder from 1 katun as found above),
 + cycle number $\times 12$ (12 is remainder from 1 cycle as found above).

In using this method 13 should be cast out at every possible stage. If the kin number exceeds 13, cast it out. If the uinal, tun, katun, or cycle number exceeds 13 cast it out before forming the product, and cast out 13 from each product before adding it to the preceding and whenever the sum exceeds 13 cast it out.

This rule is quite feasible, but a further diminution of the size of the numbers involved is possible. Consider again counting around the divisor circle. If the remainder is more than half the divisor we shall evidently arrive at the same point more quickly by counting backward the difference between the remainder and a complete revolution (*i. e.*, the divisor). This difference represents the amount the number being counted falls short of the next larger multiple of the divisor. If we are considering multiples of the number, or sums of several numbers, their backward remainders

may be multiplied and added like ordinary remainders and the amount to be counted ahead finally found by deducting their total from the divisor or from the next greater multiple of the divisor.

Now it will be noticed that the remainders from 1 uinal, 1 tun, 1 katun and 1 cycle after dividing by 13, as found on pages 53-54, are all more than half of 13. The backward remainders, $13 - 7 = 6$, $13 - 9 = 4$, $13 - 11 = 2$, and $13 - 12 = 1$ will give us smaller multipliers. This suggests the following rule: Multiply the uinal number (first casting out 13 if possible) by 6 and cast out 13; multiply the tun number (less 13 if possible) by 4 and cast out 13; add this to the preceding result and so on. Then subtract this sum from 13, add the kin number (less 13 if possible) and again cast out 13. The order of these additions and subtractions may be interchanged with advantage giving the following final form for the rule:

RULE 1,¹ FOR FINDING THE REMAINDER ON DIVIDING BY 13 A PERIOD OF TIME EXPRESSED IN THE MAYA SYSTEM

Multiply the cycle number (less 13 if possible) by 1 (that is, take it as it stands).

Multiply the katun number (less 13 if possible) by 2. Add to preceding and cast out 13.

Multiply the tun number (less 13 if possible) by 4. Add to preceding and cast out 13.

Multiply the uinal number (less 13 if possible) by 6. Add to preceding and cast out 13.

Subtract this result from the kin number increased by 13 if necessary to keep the difference positive. This will give the remainder on dividing by 13.

¹ Whether the reader has followed the preceding reasoning or not it will prove a great time saver if he is to do much work with the Maya calendar to memorize this and the following rules. Its advantage consists largely in the fact that the small size of the numbers permits of performing all the operations mentally, or writing only the previous sum to be carried along. We often use rules without being very clear as to their reasons. For instance many people who cast out 9 in our decimal system by adding the digits could not justify the process. As a matter of fact the principles involved are similar to those here used. The sequence of multipliers in this rule (beginning with the cycle multiplier and ending with that for the uinals) 1, 2, 4, 6 is an easy one to remember. It is a curious coincidence that they add up to 13.

As already pointed out it is not *necessary* to handle distances as great as cycles, because the cycle number may always be removed by subtracting calendar rounds, but it is so easy to include them in the rule that it seems desirable to do so.

Some examples will now be treated. Required the remainder on dividing 9.6.4.10.5 by 13. Perform the operations thus: $9 \times 1 = 9$, $6 \times 2 = 12$, and $9 = 21$, cast out $13 = 8$. $4 \times 4 = 16$, and $8 = 24$, cast out $13 = 11$. $10 \times 6 = 60$, and $11 = 71$, cast out $13 = 6$. 5, the kin number, is less than 6, so add 13. $13 + 5 = 18$, less 6 = 12, the required remainder.

Again, 9.17.15.16.11. $9 \times 1 = 9$. $17 - 13 = 4$, $4 \times 2 = 8$, $8 + 9 = 17$, $17 - 13 = 4$. $15 - 13 = 2$, $2 \times 4 = 8$, $8 + 4 = 12$. $16 - 13 = 3$, $3 \times 6 = 18$, $18 + 12 = 30$, $30 - 26 = 4$. 11 (kin number) $- 4 = 7$, the required remainder.

It will be noticed that the largest possible product with this method is $6 \times 12 = 72$, and the greatest possible multiple of 13 necessary for casting out therefore is $5 \times 13 = 65$. To carry out the whole process mentally involves no more labor than an ordinary short division.

The remainder so found is to be counted in the usual manner from the given day coefficient to find the new day coefficient.

3. Finding the remainder after dividing by 365, or a haab, or 1.0.5 may be done in a very similar way, except that the grade numbers cannot be diminished as before. The inconvenient form of the cycle remainder makes it perhaps a little easier to suppose the cycle number removed by deducting calendar rounds as previously explained but it is not necessary. Proceeding as for 13, the remainder for 1 kin is 1 kin, the remainder for 1 uinal is 20 kins or 1 uinal, the remainder for 1 tun is 360 kins or 18 uinals, but the backward remainder is much smaller, namely 5 kins. The backward remainder for 1 katun is 20 times this, or 5 uinals. In precisely the same way as before, in the case of 13, we get the following:

The improvements of the rule given here over Mr. Bowditch's method consist in casting out the 13 at every stage, thus bringing the work within the bounds of mental computation, in avoiding the plus and minus signs, which are confusing to a mind not mathematically trained, and in arranging the process in a rule consisting of very definite steps always to be taken in the same way.

RULE 2, FOR FINDING THE REMAINDER ON DIVIDING BY 1.0.5
A PERIOD OF TIME EXPRESSED IN THE MAYA SYSTEM

Multiply the tun number by 5 and write it as kins, reducing to uinals and kins if result exceeds 20.

Multiply the katun number by 5 and write it as uinals, reducing to uinals and tuns if result exceeds 18.

Add these two, and subtract their sum from the uinals and kins of the given distance, adding a sufficient multiple of 1.0.5. to keep the difference positive. The result will be the required remainder expressed in uinals and kins. It should be counted in this form, the uinal number being exactly adapted to counting uinals without further reduction.

This method may be extended to include cycles, thus obviating the necessity of subtracting calendar rounds by means of a table. The student may choose between the following and the calendar round table. The backward remainder from 1 katun was seen to be 5 uinals. From one cycle it is then 20×5 uinals or 100 uinals = 5.10.0. Casting out $5 \times 1.0.5$. this becomes 0.8.15. The remainder itself is then $1.0.5 - 8.15 = 9.10$. The following addition therefore to the Rule just given takes care of cycles.

Add to the previous result the cycle number times 9.10 casting out 1.0.5. from the result. As the majority of cycle numbers is 9 the amount to be added in most cases is $9 \times 9.10 = 4.13.10$ which on casting out $4 \times 1.0.5$ gives 12.10. To remember this last figure and add it will probably save time over using the calendar round table.

Appendix to Rule 2.—Multiply 9.10 by the cycle number, add the previous result and cast out 1.0.5. If the cycle number is 9 add 12.10 at once (casting out 1.0.5. if possible).

Some examples will now be considered. Required the remainder on dividing 9.6.4.10.5 by 1.0.5.

$4 \times 5 = 20$ kins	=	1.0	Given uinals and kins	10. 5
$6 \times 5 = 30$ uinals	=	<u>1.12.0</u>	Multiple of 1.0.5 to keep	
Sum		1.13.0	result positive	<u>2. 0.10</u>
			Sum	<u>2.10.15</u>
			Deduct	<u>1.13. 0</u>
				15.15
			Add 12.10 for the 9 cycles	<u>12.10</u>
			Sum	<u>1.10. 5</u>
			{ Cast out 1.0.5 for the required result	10. 0

The step marked with the brace would have been unnecessary if calendar rounds had been deducted at the start. In following the present method a change of order is perhaps better, as in the next example.

9.17.15.16.11				
$15 \times 5 = 75$ kins	=	3.15	Uinals and kins given . . .	16.11
$17 \times 5 = 85$ uinals	=	<u>4.13. 0</u>	Remainder for 9 cycles . .	12.10
Sum		4.16.15	Add $4 \times 1.0.5$	<u>4. 1. 0</u>
			Sum	<u>5.12. 1</u>
			Deduct	<u>4.16.15</u>
			Required remainder . .	13. 6

Next let us apply these methods of finding remainders to a case from the Maya calendar. Required the terminal date a distance of 9.12.8.14.1 forward from 4 ahau 8 cumhu. The remainder on dividing by 20 is the kin number 1. The day name is therefore 1 forward from ahau; that is, imix. For the remainder on dividing by 13, $9 \times 1 = 9$, $12 \times 2 = 24$, $24 + 9 = 33$, $33 - 26 = 7$, $8 \times 4 = 32$, $32 + 7 = 39$, $39 - 39 = 0$, $14 - 13 = 1$, $1 \times 6 = 6$, $6 + 0 = 6$. 1 (kin number) + 13 = 14, $14 - 6 = 8$. 8 days forward from 4 gives $4 + 8 = 12$. So far then 12 imix. For the remainder on dividing by 1.0.5.

$8 \times 5 = 40$ kins	=	2.0	Uinals and kins given	14. 1
$12 \times 5 = 60$ uinals	=	<u>3.6.0</u>	Remainder for 9 cycles . . .	12.10
Sum		3.8.0	$2 \times 1.0.5$ to make sum ex-	
			ceed 3.8.0	<u>2. 0.10</u>
			Sum	<u>3. 9. 1</u>
			Deduct	<u>3. 8. 0</u>
				1. 1

Count forward 1 uinal from 8 cumhu would be 8 pop, but we must deduct 5 kins as we pass uayeb, which gives 3 pop. Then 1 kin makes 4 pop. Result 12 imix 4 pop.

Again, let us reckon the terminal day reached by counting 9.0.19.2.4. from 4 ahau 8 cumhu. Counting forward the kin number, 4, from ahau the day name is evidently kan. Next find the remainder on dividing by 13. In writing the process the result of each step only will be written 9. 0, $9 \cdot 24 + 9 = 26 = 7$. $12 + 7 = 19$, *i. e.*, 6. $4 \text{ (kins)} + 13 = 17$. $17 - 6 = 11$. 11 days forward from 4 is 2. So far 2 kan. $19 \times 5 = 95 = 4.15$.

Given.....	2. 4
9 cycles.....	<u>12.10</u>
	14.14
Deduct.....	<u>4.15</u>
Result.....	9.19

Count forward 9 uinals from 8 cumhu, deducting 5 kins as you pass uayeb from the 19 kins, result 8 chen and 14 kins yet to go, which brings 2 yax. The whole result then is 2 kan 2 yax.

THE SECOND PROBLEM

Take up now the second problem stated above, namely, given two dates by names and coefficients, required the distance from the first to the second. It is evident that except for the fact that all complete calendar rounds must be thought of as rejected this problem is the reverse of the other. That is we must now find a distance such that the remainder on dividing by 20 (*i. e.*, the kin number) shall be what we get by counting from the first given day name to the other day name, and also such that the remainder on dividing by 13 shall be what we get by counting from the first given day coefficient to the other day coefficient, and also such that the remainder on dividing by 1.0.5. or 365 kins shall be what we get by counting from the first day in the year to the second. It will be convenient to use the following abbreviations:

For the count from the first day name to the second, X ;
 “ “ “ “ “ “ “ coefficient to the second, Y ;
 “ “ “ “ “ “ “ in the year to the second, Z .

Stated in terms of these symbols the problem is to find a distance such that the remainder on dividing by 20 (*i. e.*, the kin number) is X , on dividing by 13 is Y , and on dividing by 365 or 1.0.5 is Z . Let us try to satisfy the last two conditions first. If the distance divided by 13 leaves a remainder Y it must be possible to write it in the form $13m + Y$, where m is a whole number (the quotient). Similarly it can be written $365n + Z$, where n is a whole number. But these represent the same distance, hence $13m + Y = 365n + Z$ or dividing by 13

$$m = \frac{365n + Z - Y}{13}, \text{ which } = 28n + \frac{n + Z - Y}{13}.$$

Denote by $[Z - Y]_{13}$ the remainder on casting out 13 from $Z - Y$. Then the only part of this value of m which is not a whole number on the face of it is

$$\frac{n + [Z - Y]_{13}}{13}.$$

m however is a whole number, consequently the numerator of this must be divisible by 13. This will happen if $n + [Z - Y]_{13} = 13$ or 26 or 39 or 52, etc. Then n must be $13 - [Z - Y]_{13}$ or this number increased by a multiple of 13. The distance therefore is $365 \times \{13 - [Z - Y]_{13}\} + Z$ or this plus 365×13 or $365 \times 13 \times 2$ or $365 \times 13 \times 3$. It is not necessary to go further, for $365 \times 13 \times 4$ is a complete calendar round. What determines the proper multiple of 365×13 to add? Evidently the one remaining condition to be satisfied, that the kin number of the found distance shall equal X . Expressing distances now in the Maya notation the rule may be formulated thus:

RULE 3, FOR FINDING THE DISTANCE BETWEEN TWO GIVEN DATES

Compute $\{13 - [Z - Y]_{13}\} \times 1.0.5 + Z$,¹ where Y is the count from one day coefficient to the other, and Z is the count from one day in the year to the other, and $[Z - Y]_{13}$ signifies the remainder on casting out 13 from $Z - Y$. If this result has the correct kin

¹ The division by 13 in finding $[Z - Y]_{13}$ should be done in the Maya notation by the short division process explained on page 50.

number (*i. e.*, X , the count from the first day name to the second) it is the required distance. If not, subtract its kin number from the required one, X (borrowing 20 if necessary). The difference must be 5, 10, or 15.

$$\begin{array}{lcl} \text{If } 5 (= 1 \times 5) & \text{add } 1 \times 13 \times 1.0.5 = 13.3.5 \\ \text{" } 10 (= 2 \times 5) & \text{" } 2 \times 13 \times 1.0.5 = 26.6.10 \\ \text{" } 15 (= 3 \times 5) & \text{" } 3 \times 13 \times 1.0.5 = 39.9.15 \end{array} \Bigg\}^1$$

This will give the required distance. If Z should be less than Y it may be increased by 1.0.5 in computing $[Z - Y]$ so as to keep this difference positive.

To facilitate the use of this method it will now be given in a less exact way which is perhaps easier to remember and which is enough to recall the successive steps.

RULE 3a. The Year Position Difference Less the Day Coefficient Difference. Cast out 13. Subtract from 13. Multiply by a year, and add to the year position difference. If this is not right add enough 13 years to make it right. Test correctness by comparing kin number with day name difference.

As an example of the application of this rule let us find the distance from 2 kan 2 yax to 7 muluc 17 tzec. Here X = count from kan to muluc = 5. $Y = 7 - 2 = 5$. Z = count from 2 yax to 17 tzec = 13 uinals + 17 - 2 kins + 5 kins in uayeb = 14.0. The formula to be computed is then $\{13 - [14.0 - 0.5]_{13}\} \times 1.0.5 + 14.0$. Now $14.0 - 0.5 = 13.15$. $[13.15]_{13} = [15]_{13} = 2$. $13 - 2 = 11$. $11 \times 1.0.5 = 11.2.15$.² $11.2.15 + 14.0 = 11.16.15$. But the day name difference, X , is 5 which does not agree with this 15 kin number. Now $5 + 20 - 15 = 10$, which is 2 times 5. Add therefore 2 times $13 \times 1.0.5 = 26.6.10$ and the result is $38.5.5 = 1.18.5.5$, which is the required distance.

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¹ Of course 26 tuns and 39 tuns should be reduced to katuns and tuns to adhere to the Maya notation, but it is probably easier to remember them as written here, by means of the progressions, 5, 10, 15 for kins; 3, 6, 9 for uinals; and 13, 26, 39 for tuns.

² Multiplication of this product should be carried out in the Maya notation as explained on page 50.

CERTAIN PRE-COLUMBIAN NOTICES OF THE INHABITANTS OF THE ATLANTIC ISLANDS

By W. H. BABCOCK

THIS paper does not pretend to be exhaustive. Territorially it is confined to the eastern oceanic islands, from Iceland to the Canaries.

Of the first we have exceptionally ample information as to the settlement by the Norsemen about the opening of the tenth century and the subsequent fortunes of that turbulent but brave and intellectual northern republic. We know also that a few Irish monks had preceded them along the coast. Nansen holds that there may have been an inland Celtic colonization as well; but the local names on which he relies may be accounted for better by the presence among the Norsemen of many Irishmen and Hebrideans, both captives and free immigrants, such as arrived in the train of Queen Aud the Deep Minded of Dublin.

There are some suggestions, however, in the thirteenth century sagas which may be treated either as the offspring of fancy or as the records of last remnants of aborigines, taking refuge in the interior fastnesses of the land. Thus the well-known saga of Grettir tells how that formidable outlaw in one crisis of his fortune dwelt for a season with a family of trolls in a hidden valley of a difficult and secluded mountain. The word troll was certainly used, before and after this time, in some instances to designate the Eskimo of Greenland. It thus occurs in the Floamanna saga and other writings. This raises the possibility that there were once Eskimo in Iceland. But there is no corroborative evidence, and the passages in question are (more likely) borrowings from Norwegian tradition or fancy flights of the romancer.

The Azores seem to have been occasionally visited very early indeed; even a hoard of Carthaginian and other northern African Phoenician coins having been found on the coast of Corvo, the

nearest island to America. There are various passages in old writers of various periods, as will be seen, which may indicate later visits, also the discovery of inhabitants, but these points remain very uncertain. Nowhere is archaeologic and anthropologic research more needed, especially in the two outlying members of the archipelago.

Perhaps the most widely discussed account of an Atlantic island is what Plato declared concerning Atlantis in the fifth century before Christ, on the faith of a much earlier poem and narrative by his ancestor Solon, who in turn derived his information, as alleged, from the priests of Sais in Egypt, who were the repositories of very ancient traditions. Judging by climate and the statement of location, Madeira or some land not very far removed might be meant, though certainly conceived of as on a much larger scale than anything now existing there. Many different explanations of the legend have been proposed. About the most recent of them finds in it a distorted, exaggerated, and transplanted history of the domination and downfall of the Minoan Sea Kings of Crete. Others have assumed it to be a mere creation of the romantic fancy of Plato himself. But this seems untenable; and the general drift of opinion is toward viewing the tale as a tradition repeated in good faith and located from the beginning in the Atlantic Ocean, whatever its foundation in fact.

The tale runs as follows, using a popular English translation with some elimination and condensation:

These histories tell of a mighty power which was aggressing wantonly against the whole of Europe and Asia and to which your city put an end. This power came forth out of the Atlantic Ocean, for in those days the Atlantic was navigable, and there was an island situated in front of the straits which you call the Columns of Hercules; the island was larger than Libya and Asia put together and was the way to other islands, and from the island you might pass through the whole of the opposite continent which surrounded the true ocean. . . . Now in the island of Atlantis there was a great and wonderful empire, which had rule over the whole island and several others, as well as parts of the continent. . . . But afterward there occurred violent earthquakes and floods and in a single day and night of rain the island of Atlantis disappeared and was sunk beneath the sea. And that is the reason why the sea in those parts is impassable and impenetrable, because there is such a quantity of shallow mud in the way; and this was caused

by the subsidence of the island. . . . Because of the greatness of the empire many things were brought to them from foreign countries. There were a great number of elephants in the islands.

Numerous kinds of fruits, flowers, legumes and other vegetable productions are described as

brought forth fair and wondrous in infinite abundance. . . . They employed themselves in constructing their temples and palaces, harbors and docks, a great palace which they continued to ornament during many generations, canals and bridges, walls and towns, numerous statues of gold, fountains both cold and hot, baths and a great multitude of houses. In the mountains were many wealthy inhabited villages. The plain was rectangular and for the most part straight and oblong.

The inhabitants are represented as worshipping Poseidon, their first founder, and being ruled by his laws which were inscribed on a column erected in the center of the island.

The reference to difficulties of navigation may indicate acquaintance with the Sargasso sea of weeds beyond the Azores; but the distinctively human data belong rather to the most advanced regions of the eastern Mediterranean in Plato's time, and the elephants of Atlantis are certainly oriental, not Atlantic. On the whole we do not get the impression that Plato or Zeno or the Egyptian priests had any real information about the inhabitants of Atlantic islands.

In the first century before Christ Diodorus Siculus wrote in his *Library* what Brown's translation (in the headline of Chapter 2, Book 5) identifies as an account of "Madeira."

Over against Africa lies a very great island in the vast ocean, of many days sail from Lybia westward. The soil here is very fruitful, a great part whereof is mountainous but much likewise champaign, which is the most sweet and pleasant part of all the rest, for it is watered with several navigable rivers, beautified with many gardens of pleasure, planted with divers sorts of trees and abundance of orchards interlaced with currents of sweet water. The towns are adorned with stately buildings and banqueting houses up and down, pleasantly situated in their gardens and orchards. And here they recreate themselves in summer time as in places accommodated for pleasure and delight. . . .

The mountainous part of the country is dotted with many large woods and all manner of fruit trees. . . . There you may have game enough in hunting all sorts of wild beasts. The air and climate of the island is very healthful and

mild, so that the trees and fruit and other things that are produced there are fresh and beautiful most part of the year.

The Phœnicians . . . passed at length beyond the pillars of Hercules into the sea called the ocean and first they built a city called Gades near to Hercules' pillars and at the sea side of an isthmus of Europe. . . . The Phœnicians having found out the coasts beyond the pillars and sailed along the shores of Africa were on a sudden driven by a furious storm far off into the main ocean and after they had been under a violent tempest for many days they at length arrived at this island and thus coming to the knowledge of the nature and pleasantness of this isle they were the first that discovered it to others. . . . The Etrurians when they were masters of the sea designed to send a colony thither; but the Carthaginians opposed them, fearing that most of their own citizens should be allured by the goodness of the land to settle there and likewise intending to keep it as a place of refuge for themselves.

The climate, physical characteristics and geographical situation all seem to point to Madeira; but the human life reported has no note of authenticity. It seems to echo observations along the Mediterranean coasts of Europe.

The elder Pliny narrated not much later a Mauritanian naval expedition to the Fortunate Islands or Canaries, which met with no inhabitants, though they found a temple on an island which they called Junonia, perhaps the modern Gomera, with other necessary indications of human occupancy.

Plutarch describes an island, Ogygia, five days' sail from England and mentions a continent beyond; which statement may be reminiscent of America. But there is no certainty, and he seems to regard the inhabitants as transplanted Greeks, except the God Saturn, who lay asleep in one of the islands.

There are various other references to western lands in ancient authors and those of the dark ages and early middle ages, but these have little or no anthropologic value until we come to Edrisi.

This great Arabian geographer prepared from the reports of many investigators and from many books a world map in silver for King Robert of Sicily about 1155. He also wrote a full geographical account of the known world, illustrated by a world map, probably copied from the silver original and by many detail maps of zones called climates and divided by latitudes. Of this important work four manuscript copies are still extant, two of them being in

Paris and two in the Bodleian library in Oxford. I have obtained good rotograph copies of all the parts of maps illustrating Atlantic islands and belonging to these two Bodleian manuscripts. The divergency is great, and probably greater still from the Paris copies, to judge by the published restorations which have been attempted from time to time. Clearly no one is in a position to restore exactly what Edrisi originally showed.

There is a full but rather old translation into French by Jaubert from the two Paris manuscripts and another of 1866 by Dozy and Goeg'e of the part concerning Africa, Spain and the Atlantic islands from all four manuscripts. The latter has been usually followed by me in the quotations given below—with some slight condensation.

Edrisi tells us at the beginning of his account of the First Climate of two islands called the Fortunate Isles or El Khalidat; giving no human particulars excepting that a stone column had been found on each of them supporting a copper statue pointing to regions beyond. He reports that there was said to be six of these pedestals and statues, the first being at Cadiz. There was not known to be any inhabited land beyond. He adds that Ptolemy reckoned longitude and latitude from these islands. It may be that he had chiefly in mind Lanzarota and Fuerteventura, the two most easily accessible members of the Canary group, confusing them a little with Ferro, another island of the same archipelago farther at sea.

Later, in treating of the Third Climate, which by his system would be farther to the northward, he describes other islands, which have been sometimes understood as the Azores. But he specially states that one of these islands is near the shore of Morocco; and it seems more likely that his account skips about capriciously from one part of the eastern archipelago to another. It may be said that his hero Dzou l' Carmain is a traditional blending of Hercules and Alexander.

He tells us of the Island Sara situated near to the Sea of Darkness, where Dzou l' Carmain landed before the shadows had covered the face of the sea and remained one night; and the inhabitants attacked him and his companions with blows from stones and

wounded many. Their canine teeth project from their mouths, their eyes scintillate like stars and their cheeks look like burnt wood.

Another island of the same sea is called the island of the feminine devils, the men having no beard and the two sexes differing little in appearance. Their clothes are the leaves of trees. They speak an unintelligible language and make war on marine monsters. This may imply the use of boats. If so, it stands alone as such a nautical allusion. But perhaps the inference is unnecessary. "Monsters" would no doubt include whales, and Fayal of the Azores has continued until now a center of the whaling fishery.

Next, the Island of Delusion is of considerable size, dominated by a mountain, on the side of which live short brown men wearing a beard that descends to their knees and feeding on spontaneous vegetation such as nourish the animals. A river of sweet, fresh water descends from the mountain. The requirements of this island are met by Teneriffe, except the beards and the apparent limitation to a single stream. They may be met still more completely by Pico of the Azores; but we do not know of inhabitants there from any other source.

The Island of El Ghauer, equally large, abounds in all kinds of herbs and plants. Mentions strong places of retreat for the savages and cattle, the latter having horns of great length. Grand Canary and its caverns would suit this very well, if for the cattle we read goats—not otherwise.

The Island of Al Mustackhin, isle of the suppliants, has a citadel dominating the city, for the isle is populous, having mountains, rivers, trees, fruits and cultivated fields. Edrisi relates the victory of Dzou l' Carmain over a dragon which had been devastating this island. There seems no clue to the latter and the citadel is probably as mythical as the dragon.

Another island is named Calhan, the inhabitants of which have human forms and animal heads. Perhaps baboons or gorillas encountered on some island off the African coast may be responsible for the grotesque fantasy. In that case the island would probably belong to a more southern region than the Canaries and would be closer in shore.

Another isle is that of the two brother magicians. They were pirates. To punish them God turned them to rocks on the sea shore. They are situated opposite the port of Asafi and at a distance which permits the smoke from the isle to be seen from the continent when the air is perfectly clear.

This is related in particular by Abmed Ibn Omar, an Arabic naval commander who wished to take his fleet there and land, but death surprised him before he could do so. Information as to the naming of Asafi and other curious items is derived from the report of the Maugrurin, who sailed from Lisbon. Saffi or Asifi is about opposite the Canary Islands. Lanzasote would be nearest. The smoke of the volcano of Teneriffe may be meant.

In this sea is an island of vast extent surrounded by deep shadows. They call it the Isle of Sheep because there are enormous flocks of them. These animals are small and their flesh is so bitter that it cannot be eaten.

Edrisi again refers to the Maugrurin narrative in corroboration.

This Isle of Sheep cuts a great figure in Mediaeval romancing. In the earliest map, 1325, showing the Island of Brazil west of southern Ireland, an alternative name "Montonis" there inscribed means "rams" in Italian; unless it should rather be read Montanis for Mountains. But whether the Isle of Sheep or the Isle of Rams represents any real land is simply a puzzle unsolved. The same may be said for the strange savor of their flesh.

Near this isle is the isle of Raca or Isle of the Birds, a species like eagles which feed on marine animals.

He quotes the *Book of Wonders* for an account of an expedition sent by

a king of France to this island for the birds and for a fruit which is an antidote for poison; but the fleet was wrecked and never returned.

The Azores as an archipelago are said to have their name from the great number of hawks or buzzards found there. In one of the fifteenth century maps the Isle of Falcons appears as one of the newly rediscovered members of that widely dispersed group. Corvo, the crow, has substantially held its name since its first appearance on the maps in the middle of the fourteenth century—probably meaning cormorants. Putting these things together it

seems likely that an Isle of Birds should be looked for especially in this part of the sea. Also the Pizagano map of 1367 represents a Breton fleet undergoing disaster from shipwreck, dragons, and octopi at an island southwest of the circular Island of Brazil, which again is southwest of Ireland, the first mentioned island being also far northwest of Terceira of the Azores. It is at least possible that this may be the expedition from France referred to by Edrisi.

Edrisi's greatest Atlantic island seems to be Saisland—by one conjecture Iceland, but more probably a magnified Madeira. He says it is fifteen days in length, two in breadth. There have been civil wars there, but it is still populous with cities. This last statement has never been true of any island in the Atlantic, as far as we know. The most reasonable conclusion is that Saisland, if real, has been exaggerated and glorified in Edrisi's account quite out of recognition. The earlier description of Diodorus may have misled him.

Laca is another island of this sea. It produces a great quantity of aloes. Merchants repair thither for it and sell it to the kings of Western Morocco.

This carries us into southern waters again, but we really know nothing about Laca.

It seems idle to attempt to identify each one of Edrisi's islands and perhaps his own mind was not very clear concerning them. Arab traders from Morocco, Arab naval expeditions, French records or rumors, explorers of the Maugrurin kind and myth-makers of a much earlier time all had contributed something to his vision of the Atlantic islands, which he would like to pass on to us. But he really seems to have gathered some valuable facts. The Portuguese, when discovering or rediscovering Madeira and the Azores in the fifteenth century, did not report any inhabitants; but Edrisi did, and the natural probabilities of the situation would seem to call for them. A really thorough archaeological and anthropological search of all this group is a great desideratum, and this is especially true of Corvo, nearly halfway across the Atlantic toward America.

Edrisi has been most widely known for his narrative of the Maugrurin, sea wanderers who left the port of Lisbon, necessarily

after the Moors captured it but evidently long before the time of his writing, with the resolve to penetrate to the end of the Sea of Darkness.

They reached the Isle of Sheep, of which so much is made in so many narratives of different nations, and which may be one of the larger Azores islands. Afterward they became entangled in the weedy sea and were glad to turn southward. In the end they were detained by the chief of a populous island, who had an Arabic interpreter and reported a voyage of his father for some forty days beyond his island, without finding an end of the sea. He had them conveyed to the African coast, which was not very distant, and they found their way back to Lisbon, where a street was named after them. If any faith may be put in the tale, they had demonstrated the extension of Arab influence and language in some slight degree to the nearer members of the Canary group; but they seem to have added no other item of anthropologic value.

The Maugrurin legend itself in general outlines may very likely be true, for it was quite certain that some of the maritime Arabs would push out from the Iberian coast in a venturesome manner, drawn by the mystery of the shadowy sea and perhaps also by rumors of Christian fugitives who had sought refuge on remote islands with great wealth of every kind—legends of the Island of the Seven Cities, which endured for many centuries afterward, influencing exploration and geography, and of which no one can positively say that there is no germ of truth in them.

We do not know whence Edrisi derived this Moorish tale. It is one of his sources; but far from the only one. Obviously these were heterogeneous in nature and of quite different value. He believes in advanced civilization in some islands and of beast-headed people in others; but his varying accounts of savagery or pastoral habits, beards or smooth faces, brown skins and small stature, and of fig leaves for garments all have a ring of reality. Perhaps the first meetings of white men with insular Berbers and modifying elements are recorded in these notes of Edrisi.

The Irish Inrama or sea-sagas had a very early origin, but grew and changed during their long pre-Columbian life. They

certainly indicate a knowledge of Atlantic islands and a sense of their loveliness; but the human island population to which they introduce us is conventional and unilluminative. We hear of monasteries and hermits, of enchantresses and marooned navigators, of Enoch and Elijah on one island and the tortured Judas on another; but after much traveling among monsters and adventures we find surprisingly little to indicate observation of the real inhabitants of any real islands remote from Hibernia. The voyage of Snedgus gives us again the beast-headed people of Calhan; huge birds figure in the Voyage of St. Brandon; elsewhere in the latter some small folk are mentioned and these have been conjectured to be Eskimo; but there seems no warrant for the guess and no probability that the voyage was supposed to be in a northern direction. Bran, Maelduin, the Sons of Corra, Snedgus and St. Brandon all ranged the sea, according to romancers, and visited a multitude of islands, but they tell us very little of the real people which seems authentic, except that they kept sheep and must have lived under more or less volcanic conditions in many places.

If there are only very faint indications of pre-Portuguese populations in the Azores and Madeira, we are rewarded with quite a wealth of information as to the Canary islands from the fourteenth and fifteenth centuries.

In the year 1341, certain Italian letters, discovered long afterward among official records, were written in Florence, Italy, by certain Florentine merchants dwelling in Seville, Spain. The translation is by R. H. Major in his introduction to *The Canarien*. They relate that,

On the first of July of that same year two vessels furnished by the King of Portugal, accompanied by a smaller vessel well armed and manned by Florentines, Genoese, Castilians and other Spaniards, set sail for Lisbon and put out into the open sea. They took with them horses, arms, and warlike engines for storming towns and castles, in search of those islands commonly called the "Rediscovered." . . . They did not return until November, when they brought back with them four of the natives, a large quantity of goat skins, the fat and oil of fish, and seal skins; red wood which dyed almost as well as the vergino (Brazil wood) although connoisseurs pronounced it not to be the same; the barks of trees to stain with a red color; red earth and such other like things.

The pilot, a Genoese, estimated the distance nine hundred miles from Seville; but the distance from Cape St. Vincent was much less. Their first island, probably Fuerteventura, was one mass of uncultivated, stony ground, but full of goats and other beasts and inhabited by naked men and women, who were like savages in their appearance and demeanor. He added that he and his companions obtained in this island the greater part of their cargo of skins and fat, but they did not dare to penetrate far into the country.

Passing thence into another island (probably Grand Canary) somewhat larger than the first, a great number of natives of both sexes, all nearly naked, came down to the shore to meet them. Some of them who seemed superior to the rest were covered with goat skins, yellow and red, and as far as could be seen from a distance the skins were fine and soft and tolerably well sewn together with the intestines of animals. To judge from their gestures, they seemed to have a prince, to whom they showed much respect and obedience. Their language was soft and their pronunciation rapid and animated like Italian. On the northern coasts of the island, which were much better cultivated than the southern, there were a great number of little houses, fig trees, and other trees, palm trees which bore no fruit, and gardens with cabbages and other vegetables. Here twenty-five of the sailors landed and found nearly thirty men quite naked, who took to flight when they saw their arms. The buildings were made with much skill of square stones covered with large and handsome pieces of wood. Finding several of them closed, the sailors broke open the doors with stones, which enraged the fugitives, who filled the air with their cries. The houses were found to contain nothing beyond some excellent dried figs, preserved in palm baskets like those made at Cesena, corn of a much finer quality than the Italian, not only in the length and thickness of its grain, but its extreme whiteness, some barley and other grains. The houses were all very handsome and as clean inside as if they had been whitewashed. The sailors also came upon a chapel or temple in which there were no pictures or ornament, but only a stone statue representing a man with a ball in his hand. This idol, otherwise naked, wore an apron of palm leaves. They took it away and carried it to Lisbon. On leaving this island they saw several others. . . . They afterward saw other islands making in all thirteen, some of them inhabited and some not. . . . The languages of these people were said to be so different that those of one island did not understand another, and they had no means of communication except by swimming. . . . The four men whom they carried away were young and beardless and had handsome faces. They wore nothing but a sort of apron made of cord from which they hung a number of palm or reed fibers of a hair's breadth and a half or two hairs' breadths which made an effectual covering. They were uncircumcised. Their long light

hair veiled their bodies down to the waist and they went barefooted. . . . They did not exceed their captors in stature, but they were robust of limb, courageous and very intelligent. When spoken to by signs they replied in the same manner. . . . They sang very sweetly and danced almost as well as Frenchmen. They were gay and merry and much more civilized than many Spaniards. . . . They absolutely refused wine and only drank water. Wheat and barley they ate in plenty as well as cheese and meat which was abundant in the islands and of good quality; for, although there were no oxen, camels, or asses, here were plenty of goats, sheep, and wild hogs. They were shown some gold and silver money, but they were quite ignorant of the use of it, and they knew as little of any kind of spice. Rings of gold and vases of carved work, swords and sabres were shown to them, but they seemed never to have seen such things and did not know how to use them. They showed remarkable faithfulness and honesty, for if one of them received anything good to eat, before tasting it, he divided it into portions which he shared with the rest. Marriage was observed among them and the married women wore aprons like the men, but the maidens went quite naked, without consciousness of shame.

In 1382 an expedition under Captain Lopez was driven by storm to Grand Canary and he and twelve companions dwelt there seven years, taking care of flocks granted them and teaching the islanders Christianity. The latter, however, turned upon and killed them at the last, it is said on account of alleged hostile communications "to the land of the Christians." One of the victims first, however, wrote a warning letter which was given by a convert to the first European expedition which followed. In 1402 Bethencourt began the conquest of the Canaries, which was not completed until long after his death, by other hands and in the latter part of that century.

Gomez Eames de Azurara in 1448 compiled a narrative by one of Prince Henry's sailors of the Conquest of Guinea, reciting how a Portuguese expedition of 1443 took part in a slave raid on the Canarian island of Palma, wherein the Palma fighting men "hurled stones and lances with sharp horn points at them with great strength and precision." One of the women captured "was of extraordinary size for a woman and they said that she was the queen of a part of the island."

Azurara explains that "The Grand Canary" was ruled by two kings and a duke, but the real governors were an assembly of knights filled up by election from the sons of their own class.

Their only weapons were a short club and stones. . . . They had wheat but had not the skill to make bread, but ate the meal with meat and butter. . . . They held it an abomination to kill animals and employed Christian captives as butchers whenever they could get them. They kindled a fire by rubbing one stick against another. They believed in a God who would reward and punish, and some of them called themselves Christians.

The people of Gomera were less civilized. Their women were regarded almost as common property. . . . They made their sister's sons their heirs. They lived chiefly on milk, herbs, and roots. They ate also filthy things like rats and vermin.

The people of Teneriffe were much better off and more civilized. They had plenty of wheat and vegetables, pigs, sheep, and goats, and dressed in skins. They had, however, no houses but passed their lives in huts and caves. Their chief occupation was war and they fought with lances of pine wood made like great darts very sharp with points hardened in the fire. There were eight or nine tribes, each of which had two kings, one dead and one living, for they had the strange custom of keeping the dead king unburied till his successor died and took his place. The body of the former was thrown into a pit. They were strong and active men and had their own wives and lived more like men than some of the other islanders. They believed in a God.

The people of Palma had neither bread nor vegetables, but lived on mutton, milk and herbs. They did not even take the trouble to catch fish like the other islanders. They fought with spears like the men of Teneriffe, but pointed them with sharp horn instead of iron, and at the other end they also put another piece of horn but not so sharp as that at the point. They had some chiefs who were called kings. They had no knowledge of God nor any faith whatever.

Alvisare Cadamosto, a Venetian in the service of Prince Henry the Navigator of Portugal, as cited by Major in said introduction, reported in 1455 concerning these islands:

Four of them, Lanzarote, Fuerteventura, Gomera and Ferro were inhabited by Christians, the other three, Grand Canary, Teneriffe, and Palma by pagans. . . . They had barley bread, goats' flesh, and milk in plenty for goats were very numerous; they had no wine nor corn except what was imported, and the islands produced but little fruit. There were great numbers of wild asses especially in the island of Ferro. Great quantities of orchil for dying were sent from these islands to Cadiz and Seville and thence to other parts both east and west. Their chief products were goats' leather very good and strong, tallow and excellent cheeses. The inhabitants of the four Christian islands spoke different languages so that they could with difficulty understand each other. There were no forti-

fied places in them, only villages, but the inhabitants had retreats in the mountains to which the passes were so difficult that they could not be taken except by a siege. Of the three islands inhabited by pagans (in 1455) two were the largest and most populous of the group: namely, the Grand Canary, in which there were about eight or nine thousand inhabitants, and Teneriffe, the largest of all, which contained from fourteen to fifteen thousand. Palma was not so well peopled, being smaller, but a very beautiful island. The Christians have never been able to subdue these islands, as there were plenty of men of arms to defend them and the mountain heights were difficult of access. Teneriffe was governed by nine chiefs, who did not obtain possession by inheritance but by force. Their weapons were stones and javelins pointed with sharpened horn instead of iron and sometimes the wood itself hardened by fire till it was as hard as iron itself. The inhabitants went naked except some few who wore goat skins. They anointed their bodies with goat's fat mixed with the juice of certain herbs to harden their skins and defend them from cold, although the climate is mild. They also painted their bodies with the juice of herbs green, red, and yellow, producing beautiful devices, and in this manner showed their individual character much as civilized people do by their style of dress. They were wonderfully strong and active, could take enormous leaps and throw with great strength and skill. They dwelt in caverns in the mountains. They had some fruits, chiefly figs, and the climate was so warm that they gathered in their harvest in March or April. They had no fixed religion but some worshipped the sun, some the moon and others the planets with various forms of idolatry. Each man might have as many wives as he liked. These accounts were from the Christians of the four islands, who would occasionally go to Teneriffe by night and carry off men and women, whom they sent to Spain to be sold as slaves. Another of their customs was that when one of their chiefs came into possession of his estate some one among them would offer himself to die in honor of the festival. On the day appointed they assembled in a deep valley, when after certain ceremonies had been performed the self-devoted victim threw himself from a great height into the valley and was dashed to pieces. The chief was held bound in gratitude to do the victim great honor and to reward his family with ample gifts.

Bethencourt's expedition of 1402 was accompanied by the Chaplains Bontier and Leverrier, who prepared a joint narrative known as *The Canarien*. Their main interest is in the quarrels and adventures of the conquerors, but they supply many items about the conquered or resisting people. We are told of the great physical prowess of the King of Lanzarota, who six times freed himself from his captors, of the caverns in which the natives of that island took refuge, of the beauty and modesty of the Lanzarote women, who wear long leather robes reaching down to the ground and

most of whom have three husbands each, of the raids before this time by Spaniards and Corsicans on the people of this island. We hear also of the large stature and powerful build of the men of Fuerteventura, "who are difficult to take alive," of their notable fortifications, one being a strong wall across the island, of their attachment to their own forms of government, and the superlative quality of their cheeses. Also of the "very fine race of Ferro," both men and women, and their great abundance of dogs, sheep and goats; the delightfulness of Palma, its fine people and their exclusive flesh diet; the tallness of the Gomera islanders, their most difficult language spoken mainly with the lips as if they had no tongue; of the Teneriffe Guanche, the hardest race in all these islands; of the Grand Canary husbandmen who cultivate all kinds of grain and are enthusiastic fishers as well, whose men are handsome and well formed and whose women are beautiful. Those various accounts are mutually corroborative in their main lines, though materially differing more or less in detail even when treating of the same island, the discrepancies being in part accounted for by the lapse of time between observations and the intervening raids, importations and changed conditions. Like Edrisi's much earlier notes, they agree as to great local dissimilarities in matters of comfort and civilization; but we find no evidence that any of these island people had risen as high as the denser American populations—Inca, Aztec or Maya—or much higher than the more promising Indian tribes within the borders of the United States. They had been long separated, island from island, forgetting the art of navigation which had once brought them there, probably in at least two distinct waves, with a wide interval between them; so that the local dialects or languages were hardly mutually intelligible. This loss of navigation of course implies a considerable declension in general culture, but it must be said that we hear of no evidences of great cities and other enduring monuments of civilization such as the ancient writers quite freely depicted.

The local differences may well have been due in part to a lack of homogeneity of race. We know of many raids by white men and of some temporary colonizations lasting for years. Similar tales

are to'd of the Arabs. Major finds in the facts of divergence evidence of a subsequent Arab wave of population impinging on the Berber aborigines and obtaining control of the islands nearest the African coast, but affecting more slightly those which are remoter and greater. But this does not seem to meet all the requirements of the case and Father Espinosa who wrote while there were still living Guanche on Teneriffe, where he long made his home, contrasts the rather brown and usually nearly naked aspect of the southern shore natives with the white and rosy attractiveness of those on the northern side, whose women were beautiful. One would not think of Arabs in just that way and must be inclined to attach some weight to traditions of Gothic or other Aryan immigrants, without denying the Arabic admixture in other quarters, perhaps on a larger scale. Nevertheless it is likely that the Berber substratum remained the mass of the population.

Perhaps there is nothing about these people more distinguishing or significant than the use habitually made of caverns in some of the chief islands, and the preservation of the dead as mummies especially on the most populous island, Teneriffe. In Lanzarota the caverns were mainly temporary places of refuge in time of sudden danger. In Teneriffe some of them were more regular and permanent abodes, others served as mortuaries for the deposit of dead generations after the manner of the catacombs. The caverns seem to link them, perhaps by coincidence only, with the troglodytes of northern Africa and certain Indian tribes of our cordilleras in the southwest. The habit of preserving corpses as mummies in similar fashion links them on the one hand to the Nile and on the other to the land of the Inca. But again it may be a mere matter of chance or the result of like requirements, conditions, and impulses in spots.

Taking a general survey of the field, we have no evidence of human occupancy in Iceland other than Celtic and Norwegian, though there are faint indications pointing that way; that the case for the Azores and Madeira is much the same, apart from occasional European visitors from the mainland, though certain matters of nomenclature and the probabilities of the situation

suggest an early native population; but that there is ample and detailed information concerning a native population of the Canaries who must have reached their island homes by navigation in remote times, who occupied stations well advanced toward America on the route first followed by Columbus and who present many traits, customs and characteristics which remind one of the North American Indians. But of course these may have grown out of similar conditions of primitive tribal life and may not imply any community of descent.

WASHINGTON, D. C.

ANTHROPOLOGY AT THE PHILADELPHIA MEETING
WITH PROCEEDINGS OF THE AMERICAN
ANTHROPOLOGICAL ASSOCIATION
FOR 1917

By WILLIAM CURTIS FARABEE

THE nineteenth meeting (sixteenth annual) of the American Anthropological Association was held December 27-28, 1917, at the University Museum, Philadelphia, in affiliation with the American Folk-Lore Society.

The council meetings were attended by Kroeber, Gordon, Swanton, MacCurdy, Goddard, Moorehead, Peabody, Michelson, Nelson, Parsons, Hrdlička, Wissler, Speck, Goldenweiser, Wardle and Farabee. President A. L. Kroeber occupied the chair.

REPORT OF THE SECRETARY

The Proceedings of the last annual meeting of the American Anthropological Association were published in the *American Anthropologist* for January-March 1917. There has been no special meeting of the Association nor of the Council during the year.

One member has died, Dr. Juan B. Ambrosetti of Buenos Aires. Many of the members will remember with pleasure Dr. Ambrosetti's visits to this country in 1902 and 1912. South America has lost in his death one of its leading anthropologists and all Americanists a distinguished fellow-worker.

The Secretary, Alfred M. Tozzer was called into the service of the Government as Captain in the Aviation Section, July 1917, and William Curtis Farabee was appointed by the Executive Committee to fill out the unexpired term.

The fifty-six applicants for membership submitted by the Secretary were all elected, making a total membership of 455. The revised list of members with addresses is published elsewhere in this issue.

REPORT OF THE TREASURER

RECEIPTS AND DISBURSEMENTS, JANUARY 1, 1917, TO DECEMBER 31, 1917.

Receipts

Balance on hand, January 1, 1917			\$195.75
Anthropological Society of Washington:			
Balance unpaid, January 1, 1917.....	\$	47.25	
<i>American Anthropologist</i> , vol. XVIII, no. 4.....		47.25	
<i>American Anthropologist</i> , vol. XIX, nos. 1-3....	127.81	\$	222.31
Less amount still unpaid.....		87.56	134.75
American Ethnological Society:			
<i>American Anthropologist</i> , vol. XVIII, no. 4.....		63.88	
<i>American Anthropologist</i> , vol. XIX, nos. 1-3....	212.64		276.52
Annual membership dues:			
Account of 1915.....		6.00	
" " 1916.....		55.00	
" " 1917.....		1,873.00	
" " 1918.....		154.00	2 088.00
Subscription to <i>Anthropologist</i> , unpaid Jan. 1, 1917.			4.05
Sale of publications:			
Amount unpaid January 1, 1917.....		34.47	
Less amount still unpaid.....		30.85	3.62
Current sales.....		193.14	
Less amount still unpaid.....		41.78	151.36
From Yale University, for <i>Memoirs</i> , vol. III, no. 2....			518.60
Gift to the Association from Prof. H. Montgomery...			4.00
Miscellaneous, including overcharges.....			16.79
Total receipts.....			\$3,393.44

Disbursements

Printing and distribution of publications:

Amount unpaid January 1, 1917.....	\$1,392.60		
<i>American Anthropologist</i> , vol. XVIII, no. 4.....	521.72		
<i>American Anthropologist</i> , vol. XIX, nos. 1-3....	1,111.83		
<i>Memoirs</i> , vol. III, nos. 2-4.....	1,227.49		
<i>Memoirs</i> , vol. IV, nos. 1-2.....	427.71		
	4,681.35		
By credit, sale of old metal, etc.....	64.27	\$4,617.08	
Less amount still unpaid.....		2,117.08	2,500.00

Illustrations for publications:

<i>American Anthropologist</i> , vol. XVIII, no. 4.90	
<i>American Anthropologist</i> , vol. XIX, nos. 1-4.	<u>155.87</u>	<u>156.77</u>
<i>Memoirs</i> , vol. III, no. 4.	10.98	167.75
Expenses of the Editor.		348.95
Expenses of the Treasurer.		73.48
Expenses of the Secretary.		33.70
Miscellaneous, including refund of overcharges.		<u>9.85</u>
Total disbursements		3,133.73
Balance in Washington Loan & Trust Co.		<u>259.71</u>
		\$3,393.44

PERMANENT ACCOUNT

Receipts

Previously acknowledged.	\$1,200.00
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Disbursements

Loan to General Fund.	\$1,100.00
Balance in Munsey Trust Company.	<u>100.00</u>
	\$1,200.00

NEIL M. JUDD, *Treasurer*.

The accounts of the Treasurer, Neil M. Judd, have been examined and found correct.

Signed:

TRUMAN MICHELSON

A. HRDLIČKA.

Auditing Committee.

WASHINGTON, D. C.,

January 2, 1918.

REPORT OF THE EDITOR

There have been issued this year four numbers of the *American Anthropologist*; namely, the fourth number of volume 18 and the first three numbers of volume 19. No. 4 of the current volume is entirely in page proof with the exception of the Index which is now being made up into pages. Volume 19 contains, with the exception of the Index, 591 pages and xv plates. The *Memoirs* were quite behind a year ago, since which time there have been issued two *Memoirs* of volume III and two numbers of volume IV. The third and fourth numbers of the current volume are now printing and

will be issued within a few days. The size of the numbers of the *Memoirs* is rather difficult to control because the longer papers are segregated for issue in the series and they vary considerably in length. Dr. Hatt's *Memoir* which was no. 3 of vol. III was 101 pages. No. 4 of this volume was a rather long article by Dr. Thurnwald and contains 141 pages. Volume IV will contain 327 pages, exclusive of the Index. The fact that the entire volume is without illustrations has very largely influenced the cost of the volume.

Valuable papers are constantly being offered both for the *Memoirs* and the *American Anthropologist*. It has been necessary to decline certain of these because of the excessive cost involved in their publication. For example, a paper apparently of great value dealing with the statistical measurements of women has been postponed from time to time, hoping means for publication might be available from outside sources. The present size of the *Anthropologist* is just about sufficient to care for the more important articles for which the Association should feel itself particularly responsible because either of the character of the articles, the circumstances under which they are presented, or the relationship of the authors to the Association. The Editor has tried to divide the available space justly between the various subjects and interests represented by the members.

Considering the present condition of the finances of the Association, the high cost of the production of the publications, and the uncertainty of the era under which we are existing the Editor feels compelled to recommend certain economies. Considering our obligations to a large number of institutions who have become members of the Association, it does seem that we can discontinue the publication of the *Memoirs* or reduce the number of issues. With justice however the number of pages can be considerably reduced. As long as present circumstances continue it is recommended that the *Anthropologist* be kept to 500 pages per volume, and the *Memoirs* to as small a compass as is possible and still issue four numbers, that is from 150 to 250 pages per volume.

PLINY E. GODDARD, *Editor*.

The Council took affirmative action on the following:

1. That the committee consisting of Boas (Chairman), MacCurdy, Goddard, Dixon, Tozzer, Hooton, Kroeber, and Speck, on "Teaching of Anthropology in the United States" be made permanent and that they make annual reports to the Council. That MacCurdy publish his report on "The Number of Institutions in the United States giving Instruction in Anthropology" at the earliest opportunity.

2. That the question of the finances of the Association be referred to the Executive Committee with power to act.

3. That "The American Anthropological Association notes with satisfaction the official recognition of Anthropology by the Government of the United States by the appointment of a Committee to represent that subject on the Council of National Defense. It is the belief of the Association that the services of Anthropology may be useful to the Government at this time and notes with satisfaction that the Committee has already made practical suggestions for the guidance of the Government along several lines. The Association stands ready to cooperate with the Committee of the National Research Council on behalf of the Government."

4. That the report of the Committee on Nominations, consisting of Swanton, Speck, and Goddard, be accepted. The election of officers resulted as follows:

President: A. L. Kroeber, Affiliated Colleges, San Francisco.

Vice-President, 1918: B. Laufer, Field Museum of Natural History.

Vice-President, 1919: John R. Swanton, Bureau of American Ethnology.

Vice-President, 1920: George Grant MacCurdy, Yale University.

Vice-President, 1921: Aleš Hrdlička, U. S. National Museum.

Secretary: Alfred M. Tozzer, Harvard University.

Acting Secretary: William C. Farabee, University of Pennsylvania.

Treasurer: Neil M. Judd, U. S. National Museum.

Editor: Pliny E. Goddard, American Museum of Natural History.

Associate Editors: John R. Swanton, Robert H. Lowie.

Executive Committee: The President, Secretary, Treasurer, and Editor (*ex officio*), Clark Wissler, Edward Sapir, G. B. Gordon.

Council: F. Boas, W. H. Holmes, F. W. Fewkes, R. B. Dixon, F. W. Hodge, A. L. Kroeber, George B. Gordon, B. Laufer, John R. Swanton, G. G. MacCurdy, A. M. Tozzer, Neil M. Judd, P. E. Goddard (*ex officio*); Alice C. Fletcher, C. P. Bowditch, S. Culin, R. H. Lowie, C. H. Hawes, E. Sapir, N. C. Nelson, H. Bingham, J. A. Mason, H. K. Haeberlin, E. W. Gifford, Elsie Clews Parsons, J. P. Harrington (1918); A. E. Jenks, S. A. Barrett, W. Hough, A. Hrdlička, B. T. B. Hyde, C. Wissler, F. G. Speck, A. A. Goldenweiser, E. A. Hooton, A. V. Kidder, F. C. Cole, L. Spier, L. J. Frachtenberg (1919); Byron Cummings, W. C. Farabee, G. G. Heye, H. J. Spinden, S. M. Barbeau, W. D. Wallis, A. B. Lewis, S. Hager, Miss H. N. Wardle, Theodoor de Booy, F. H. Sterns, S. K. Lothrop, R. T. Aitken (1920); W. C. Mills, H. Montgomery, C. B. Moore, W. K. Moorehead, E. K. Putnam, C. Peabody, C. C. Willoughby, T. Michelson, A. B. Skinner, M. H. Saville, E. W. Hawkes, Louis Sullivan (1921).

The President, Professor A. L. Kroeber, appointed the following committees:

Committee on Program: R. B. Dixon (chairman), Elsie Clews Parsons, Aleš Hrdlička, A. A. Goldenweiser, Berthold Laufer, P. E. Goddard (*ex officio*).

Committee on Finance: Charles Peabody (chairman), Edward E. Ayer, Charles P. Bowditch, William H. Furness, George G. Heye, Clarence B. Moore, Homer E. Sargent.

Committee on Policy: Franz Boas (chairman), Roland B. Dixon, J. Walter Fewkes, W. H. Holmes, Robert H. Lowie, George G. MacCurdy, A. M. Tozzer.

Committee on Publication: A. L. Kroeber (chairman *ex officio*), Hiram Bingham, Stewart Culin, P. E. Goddard (secretary *ex officio*), A. A. Goldenweiser, G. B. Gordon, Walter Hough, Neil M. Judd, F. W. Hodge, Berthold Laufer, E. Sapir, M. H. Saville, John R. Swanton, A. M. Tozzer, W. H. Holmes, R. B. Dixon, F. Boas.

To represent the Association on the Council of the A. A. A. S.:
W. H. Holmes and Franz Boas.

5. That Dr. A. Hrdlička's name be submitted to the American Association for the Advancement of Science for election as Vice-President of the Section H.

6. That the Association hold its next regular meeting at Boston.

7. That "The American Anthropological Association and the American Folk-Lore Society express their appreciation of the hospitality extended by the University of Pennsylvania in tendering the use of buildings for the holding of their scientific meetings and for the luncheon and supper-smoker on Friday, December 28. The thanks of the Societies are also given to Dr. and Mrs. W. P. Wilson for the charming Cliff Dwellers' Luncheon at the Commercial Museum on Thursday, December 27.

ADDRESSES AND PAPERS

Dr. George Byron Gordon, Director of the University Museum, delivered the opening address. Professor A. L. Kroeber's Presidential address before the Association was entitled "Centers and Areas of Culture on the Pacific Coast."

The list of authors and titles of papers presented follows:

- COLTON, H. S. Some Small House Ruins in the Coconino Forests, Arizona.
DAY, LEWIS CLINTON and STAMP, HARLEY. The Treatment of Makis Nez Batcilla Bega of Se Le Daskon.
FARABEE, W. C. The Marriage of the Electric Eel.
FRACHTENBERG, L. J. (a) What are the true Criteria for establishing Linguistic Relationships? (b) Abnormal Types of Speech in Quileute. (c) The Eschatology of the Quileute Indians.
GODDARD, P. E. The Criteria of Genetic Linguistic Relationship.
GOLDENWEISER, A. A. Form and Content in Totemism. The Mind of the Eskimo.
HRDLÍČKA, A. Report of the Committee on Anthropology of the National Research Council. The Growing Need of a Separate American Journal of Physical Anthropology.
KROEBER, A. L. Pre-American Culture of the Great Basin as Represented by Collections from a Nevada Cave.
MACCURDY, G. G. Surgery among the Ancient Peruvians.
MERWIN, B. W. Notes on Chiriqui Pottery Design.

- MICHELSON, TRUMAN. Potawatomi Notes. Hartland's Theory of the Priority of Matrilineal Kinship.
- PARSONS, E. C. Christian Rites in Zuñi Ceremonialism.
- SPECK, FRANK G. The Functions of Wampum among the Northeastern Algonkins. Bird Lore of the Northeastern Algonkins.
- STAMP, HARLEY. Northwestern and Siberian Folk Tales.
- SWANTON, J. R. Resemblances between the Chitimacha and Tunica Languages.
- WARDLE, H. NEWELL. Iron-ore Artifacts from Alabama. Note on the Ground Hog Day Myth and its Origin.
- WILDER, H. H. Restorations.
- WILSON, L. L. W. Report on Excavations at Otowi, New Mexico, Summer of 1917.

BOOK REVIEWS

METHODS AND PRINCIPLES

Culture and Ethnology. ROBERT H. LOWIE. Douglas C. McMurtrie:
New York, 1917. 190 pp.

Although the methods and principles of modern ethnology, as conceived and cultivated in America, are well known to the small band of workers, a coherent and concise summary of the ideas for which the science of ethnology stands, and which could serve for the information of a wider public, has hitherto been lacking. This long-felt want is felicitously supplied by Dr. Lowie's booklet which has grown out of a series of lectures delivered in 1917 in the American Museum of Natural History. The cause of ethnology is here pleaded with eloquence and well-balanced judgment, and a difficult subject is presented in an attractive and lucid exposition. This work will form an excellent introduction and guidance to the raw recruit along the thorny path of ethnological research, and it will be read by the veterans with as much pleasure and profit. It should be the permanent possession of every one, together with Boas's *Mind of Primitive Man* and Wissler's new book on *The American Indian*. So many confusions, misconceptions, and prejudices in regard to what ethnology constitutes are still prevalent, even in the minds of scientists in general, especially among historians, orientalisists, art-students, etc., that a book like this one will be warmly welcomed and will help to clarify the atmosphere. Dr. Lowie's universal reading, his keen analytic sense that goes straight at the root of things, and his gift of clear representation, insure his pages a piquant flavor and the constantly growing sympathy of the reader.

In his first lecture, the author endeavors to determine the relation of culture to psychology and to defend the rights of ethnology as an independent science. Psychology, as he aptly expresses it, is as impotent to reduce to really interpretative psychological principles the subjective aspect of cultural phenomena as it is to explain the historical sequence of events, and the principles of psychology are as incapable of accounting for the phenomena of culture as is gravitation to account for architectural styles. Over and above the interpretations given by psychology, there is an irreducible residuum of huge magnitude that calls for special

treatment and by its very existence vindicates the *raison d'être* of ethnology; but we need not eschew any help given by scientific psychology for the comprehension of specifically psychological components of cultural phenomena. At this point I wish the author might have made some reference to the growing importance of sexual psychology without the aid of which numerous factors in the life of all peoples cannot adequately be grasped. No sinologue, for instance, has ever been able to advance a correct explanation of the reasons why the Chinese bind (or used to bind) the feet of their women: this is a sex problem to which no historian or ethnologist (not to speak of the Chinese themselves) can make an answer. Moreover, it seems to me that for practical working purposes we are compelled to postulate the existence of a social or national psyche, whether this be correct in a strictly scientific sense or not, this psyche to be conceived, of course, as being composed of ideas and habits historically acquired and developed, and accordingly as a changeable quantity. It is a commonplace observation that the Japanese adopted and absorbed the fundamental principles of Chinese civilization; it is still more interesting to note what they did not adopt from their superior neighbors: they did not take over their costume, their domestic architecture, their furniture and kitchen, sitting on chairs and at tables (in fact retained all their ancient material possessions relative to domestic life), foot-binding, consumption of opium, not to mention numerous other customs and practises. An answer to this why has not yet been given by any ethnologist or historian of Japan. Why did Christianity conquer the peoples of the Indo-European stock, while it did not appeal to the Semites in the midst of whom it was born? Why could it never obtain any permanent influence on any nation of Asia? Why is Buddhism, the product of an Aryan society, extinct in India, the land of its birth, while it flourishes among so many non-Aryan peoples of central and eastern Asia? Or why do the tribes of Africa so easily become Mohammedanized? For each of these questions, hundreds of historical facts can be marshalled to state the merits of the particular case; but these do not bring us nearer to a satisfactory explanation. Here, and in similar problems, it seems to me the application of psychological methods or viewpoints will remain unavoidable. On the other hand, each cultural fact reacts on the tribal or national psyche, and these effects have as yet been little studied. To illustrate, among the European and many Asiatic tribes family life has always centered around the fire of the kitchen or tent; the kitchen is still the bond that unites the members of the family in our civilization (as far as they are not cor-

rupted by the institution of the family hotel). The formation of the family in any other way seems almost inconceivable to us. Yet the Chinese in this respect are radically different from others. The kitchen never formed the center of their house; on the contrary, it was always far removed from it as a separate building and entrusted to the care of male servants. There are no female cooks, and the mother of the house avoids the kitchen, where the family never assembles. While the organization of the Chinese family outwardly is very much like our own, yet the forms of family life and the psychical relations of the family members are diverse, and this difference seems to be directly traceable to influences of material causes, as given by the arrangement of habitations and the location of the kitchen.

The second lecture, "Culture and Race," forms especially attractive reading, and there is no doubt that every ethnologist will subscribe to the author's clear exposition and his statement of the relation of culture to racial traits: culture cannot be adequately explained by race, and the same race varies extraordinarily in culture even within a very narrow space of time. The Chinese and some of our American Indians, such as the ancient Central Americans and Peruvians, did attain a very high level which may be equated with that of Europe at a relatively recent period. No term has been more misused and sinned against than the catchword "racial mentality." The anthropologist is inclined to assume that mental powers are fairly uniform through mankind, but work in different directions or are projected on different lines of activities. The limitation of activities in a certain people is not the consequence of a limitation of mentality, but merely the outcome of historical agencies and traditional training based thereon. In judging nations we are just as perverse as in judging individuals: both are judged by the average man from their outward success, while, of course, success is not necessarily caused by a higher state of mentality. The best exposure of this shallowness of judgment is to the credit of a Japanese statesman, who during the Russo-Japanese war, when his country held the attention of the world, remarked bitterly, "When we were a nation of artists you called us barbarians; now that we kill men you call us civilized."

In his third lecture Dr. Lowie discusses the relation of culture to geographical environment. Here he is particularly fortunate in the choice of his illustrations by contrasting the cultures of the Hopi and Navajo, which are thoroughly disparate despite the fact that both tribes have occupied for a long period the same part of northeastern Arizona, or by characterizing the Chukchi and Eskimo, the Bushmen and Hotten-

tot. Several interesting sidelights fall in this connection on the history of domestications. The fact that environment has a rather insignificant share in the formation of culture is patent to every one who can think objectively; nevertheless, in the face of the many pretensions made by that infertile pseudo-science, anthropogeography, it becomes our duty to antagonize its hollow dogmas on every suitable occasion. Dr. Lowie very well sums up his standpoint by saying that environment cannot explain culture because the identical environment is consistent with distinct cultures; because cultural traits persist from inertia in an unfavorable environment; because they do not develop where they would be of distinct advantage to a people; and because they may even disappear where one would least expect it on geographical principles.

Psychology, racial differences, geographical environment, are all inadequate for the interpretation of cultural phenomena. Culture is a thing *sui generis* which can be explained only in terms of itself. With this motto in mind, the author finally attacks in his fourth chapter the determinants of culture. He points out the great importance of cultural diffusion and assimilation, as, for instance, in the propagation of maize, the adoption on the part of the white settlers of the entire complex of aboriginal maize cultivation (tobacco and tea are as striking examples), the early traffic in bronze and amber, and the close connection and interrelations of the ancient Asiatic civilizations. Contact of peoples is thus an extraordinary promoter of cultural development. The theory of cultural evolution, to my mind the most inane, sterile, and pernicious theory ever conceived in the history of science (a cheap toy for the amusement of big children), is duly disparaged. It is worthy of especial mention that Dr. Lowie (p. 89) recedes to some extent from his former standpoint taken on the subject of convergencies. The majority of these now appear to him not genuine, but false analogies due to our throwing together diverse facts from ignorance of their true nature. I hope the author will discuss this point somewhat more fully in the near future for the benefit of his fellow-workers. What he has to say about survivals, rationalistic explanations, and the determinants of culture in general, belongs to the best portion of the book and will hardly provoke any contradiction. Culture cannot be forced into the straitjacket of any theory whatever it may be, nor can it be reduced to chemical or mathematical formulas. As nature has no laws, so culture has none. It is as vast and as free as the ocean, throwing its waves and currents in all directions. It is absurd to seek the origin of civilization in any particular region or to trace it to a single nation. In its present aspects, culture is

the common good of mankind, the product of human thought of all ages. It is something above or below the nations, but not a thing of their own. There is no nation (inclusive of primitive peoples) that has a culture entirely evolved from its own resources; there is no living nation that has a ghost of a claim to the generation of any fundamentals of culture. Our present and our future lie in our past. All that the practical investigator can hope for, at least for the present, is to study each cultural phenomenon as exactly as possible in its geographical distribution, its historical development, and its relation or association with other kindred ideas. The more theories will be smashed, the more new facts will be established, the better for the progress of our science.

The last chapter of Dr. Lowie's work is taken up by a thorough discussion of kinship terminologies, added as a concrete illustration of the methods propounded in the four preceding lectures. This subject is particularly his own domain, and as is well known, has elicited from his pen many contributions of permanent value.

B. LAUFER.

Organic Evolution. A Text-book. RICHARD SWANN LULL. The Macmillan Company: New York, 1917. XVIII, 729 pp., XXX plates, 253 figures. Price \$3.00 net.

The addition of another book to the extensive literature on evolutionary biology is justified by the fact that the author is a paleontologist. Of late, paleontology has fallen into disfavor with experimental biologists and students of heredity. The reason for this is, that paleontologists, seeing at every hand animals in complete agreement with their environment, have shown a tendency to describe the evolution of different organisms in terms which, though they cannot be called Lamarckian, still savor of the Lamarckian theory. Dr. Lull states his attitude as follows:

The geologic changes and the pulse of life stand to each other in the relation of cause and effect. This statement does not, however, imply the acceptance of the Lamarckian factor any more than that of natural selection, for whether the influence of a changing environment acts directly upon the creature's body, or indirectly through induced habit, or, whether it merely sets a standard to which animals must conform if they would survive, matters not; the fundamental principle remains that changing environmental conditions stimulate the sluggish evolutionary stream to quickened movement.

As the title implies, the scope of the book is unusually comprehensive and it is necessarily somewhat of a compilation. Several chapters are

almost entirely direct quotations. The clearness of the various chapters is inversely proportional to the amount of quoted matter.

One half of the book deals with the principles of biology. A separate chapter deals in turn with classification of organisms, distribution in space, both geographical and vertical, and distribution in time. The second part deals with the mechanism of evolution: natural, sexual and artificial selection, variation, mutation, heredity, acquired characters, etc. The remainder of the book deals with the evidences of evolution. After discussions on coloration, mimicry, parasitism and adaptation to various modes of life the author takes up in order three great lines of evolution culminating in the cephalopods among mollusca, the insects among arthropods, and mammals among vertebrates.

Doctor Lull has been very successful in preventing a book with such range from becoming a mere outline. Few books present such a wealth of material in such a readable form. The author's distinct contribution to the book, aside from his remarkable synthetic treatment of the whole subject, is his choice of examples illustrating the various phases of the subject. The animals named are, for the most part, the more common and well-known forms. The less familiar and fossil forms are shown in an adequate number of plates and figures which are a feature of the book.

The sections dealing with man are somewhat after the style of Huxley. The author's explanation of the loss of hair on the human body as being due to the use of clothing is perhaps not convincing in view of the fact that among modern men those least burdened with clothes are most glabrous. Nor, since the work of Von Luschan, Fishberg and others, do we think the "purity of the Hebrew race" is a very good example of the effect of segregation on natural selection. But errors of fact are very rare and the deductions sound and logical.

The illustrations and well-selected bibliography make it valuable as a text-book, especially in those schools which do not have access to the actual paleontological specimens. The clear and interesting presentation also adapts it to the general reader. No one working in the field of biology can ignore the findings of paleontology and the significance of their bearing on evolution. As the author justly claims, the final proofs of the evolutionary theory rest upon the documentary evidence which, in this instance, paleontology alone can furnish. It is to be hoped that this book will do much towards raising paleontology to its legitimate place among the biological sciences.

LOUIS R. SULLIVAN

NORTH AMERICA

Zuñi Kin and Clan. A. L. KROEBER. (Anthropological Papers of the American Museum of Natural History, vol. XVIII, pt. II, pp. 39-204, 3 text figures and 9 maps.) New York, 1917.

This paper opens a new chapter in the scientific study of American Indian society, and constitutes a first installment toward the filling of a long-felt need. With few exceptions previous discussions of the social constitutions of Indian tribes have merely recorded the existence of such and such social features, the number and names of clans, the terms of relationship, the number and names of fraternities, and so on. Here at last we have a quantitative study of these features, a consistent endeavor to place the exact significance of each element in the lives of the collective body. It is true that numerous elaborate studies of the rituals and secret societies have been published but these have usually been given from the native point of view, the place which they occupy in theory, not that which they occupy in fact. Thus in many cases we have, not a statement of what the social and ceremonial organization of the tribe is, but of what it ought to be, a condition perhaps never attained. Of course the theoretical view is a valuable one if it is correctly represented but our understanding of the entire organism of the tribe can never be complete until we can compare it with its actual expression in the lives of the people.

Take clans for instance. Many field workers have been content with a mere enumeration of the number of clans, perhaps distinguishing only those known to be extinct. Sometimes the part played by these in the ceremonies is added, but usually this is rather from the point of view of the ceremony than from that of the clan. The quantitative method considers much more than this. It determines, not merely the numbers and names, but the relative strength in individuals, the geographical distribution, relative ceremonial value, and so on. For instance, how illuminating it would be if we had a complete gentile census of the Chippewa Indians, band by band, so that we could form a mental map containing this data! Such mapping is similar to that made familiar by the investigations of population now so popular among social workers, though the social worker's mind is rather on the future and the ethnologist's on the past.

Professor Kroeber's paper is essentially a social analysis of the kind indicated. Although embodying mainly a record of facts it nevertheless expresses by its very arrangement a theory — or perhaps I should rather

say view — to which its author has been led by his own investigation. This view is well summarized in his introduction and indeed in the opening sentences,

The foundation of Zuñi society is the family. Life centers about the house. The clan is above all a ceremonial institution.

And what follows is simply an elaboration of these statements. In contravention of those speculators who postulate the abnormal as a precursor of the normal, and hang upon the seemingly unusual elements of aboriginal life, such as the clan and moiety, a fantastic psychology, Professor Kroeber shows us a mental attitude toward family, friends, and neighbors, recognizable as human, one which we can understand. He says:

With the view that the present state of Zuñi society is an altered one, and that it was preceded by a condition of the predominance of clan over family, it is . . . vain to quarrel. If any one finds it more profitable to demonstrate that such and such must have been the practices of this or that or all nations at some time before we have cognizance of them, rather than to understand and weigh in just balance their manners within the historic period, that satisfaction cannot be denied him: provided he does not proclaim or assume that the rearing of such hypothetical dogmas is the justifying purpose, or the ultimate goal, of ethnology and history. Yet, it is also justifiable that those not infected with such theories should exact much and specific evidence before inclining any favor to the view that the fundamental organization of the society of the Zuñi and similarly constituted peoples was once on a clan basis.

In accordance with his conclusion as to the fundamental nature of kinship he deals with this first, giving a detailed account, illustrated by an actual genealogy, of the various terms of relationship. This is followed by a short section on "The House and Marriage", which accentuates feminine ownership of the house as the central point of the Zuñi matriarchate, but at the same time calls attention to the fact that in consequence of this,

the position of woman is not materially different from that which she occupies in nations of non-matriarchal institutions,

and finally, — evidently in answer to those theorizers who associate a clan organization with group marriage, — he emphasizes the essentially monogamous character of Zuñi marriages.

The third, and by far the longest section deals with clans, which Professor Kroeber examines from every possible point of view, their number and names, connection with marriage regulations, association

into phratries and so forth, subdivisions, size, internal government, religious and governmental functions, together with a general estimate of the entire Pueblo clan system. In the last section is a description of the present condition of Zuñi town as illustrated by an excellent map from a survey made for the author in 1915 by Mr. Mark Bushman of Gallup. The changes undergone by the structures generally and the religious structures in particular are here discussed and finally there is an examination into the location of the town of Hallonawa which is mentioned in early Spanish narratives; this Professor Kroeber concludes to have been, not the place now so called on the south side of the river, but at or near where Zuñi is today.

Too much praise can hardly be given to the idea embodied in this work or to the general method in which it has been executed. Doubtless students better acquainted with the Pueblo region than the reviewer would find details to criticize, but as a whole we have a solid and important contribution to the study of aboriginal Indian society.

In one particular, however, it appears to the reviewer that some injustice has been committed by Professor Kroeber, namely in his treatment of the evolution of the Hopi state as presented by Dr. Fewkes. It is true that a common source of danger is the tendency to assume from native tradition or native custom in the present a past social condition which is purely hypothetical; still it is going quite too far to conclude that such data can give absolutely no valuable hints as to past conditions. If ethnology is of no assistance in the reconstruction of past conditions, it is reduced to the level of a description of more or less curious and amusing phenomena, or else to a study of social psychology. I proceed to a discussion of this point.

In the first place Professor Kroeber attributes to Dr. Fewkes the theory that each Hopi clan is descended from a former village community, with which incidentally what later became the clan ritual was associated as a community ritual (p. 142). What Dr. Fewkes actually does claim on the basis of native tradition and other evidence, is that the Hopi grew up as the result of a union of different *groups of clans*, the three most important of which came from the north, south, and east respectively. Secondly, Professor Kroeber's critical comparison of Pueblo clans has shown him that practically the same system of clans is found throughout all of Pueblodom, which he thinks would not have been the case had certain clans come to Hopi from one quarter and certain from another. If this similarity in number and names of clans, agreeing with well-known similarities in culture, were accompanied by a

similarity in language, Professor Kroeber's argument would be decidedly strong. But if the immigration of a set of clans from the Paiute country be disproved by the fact that the same clans are found on the Rio Grande, how does Professor Kroeber account for the fact that Hopi is plainly a Shoshonean dialect, and that the remainder of the Pueblo Indians are divided among three stocks quite distinct from Shoshonean? Plainly we must suppose that from one side or the other, probably from both, there has been a diffusion of clans. If at the time of their immigration the Hopi did not have clans, we may grant that clans may have been superposed upon them in such a manner that none of the original elements could afterward be discriminated, but this is as speculative as anything Dr. Fewkes proposes. The facts to be explained are these. The Hopi are a people having a Pueblo culture like the Zuñi, Keres, and Tewa peoples, but a Shoshonean language unlike any of them, and on the other hand they are the only people employing a Shoshonean dialect to have such a culture. Do not these facts at once suggest that the condition has been brought about by intimate contact between one people which had the culture and another which had the language? And when a native tradition is found which seems to square with the expected in these very particulars, why should we not consider it a more probable explanation than to suppose that a single body of Shoshoneans had been violently Puebloized in culture or a single body of Pueblo Indians Shoshonized in language, suppositions which have not even the merit of being *equally* plausible? And if we find native tradition agreeing with the expected in one particular, why not assume its correctness in the threefold origin of these people?

The fact that the same clan names are found throughout Pueblo territory is no insuperable obstacle to such a supposition. In the first place the number of names likely to be selected to designate clans is not unlimited and we may confidently predict independent origins for some of them, but, more than that, the removal of one woman to another town transplants the clan along with her and no very great period of time need be required for all of the clans in a given totemic area to spread throughout that area. Finally there exists such a thing as assimilation of clans through which clans of different names in different towns will consider themselves related and in time change, or exchange, their names. Thus each of the seven Natchez clans is today identified with a corresponding clan among the Cherokee although not half of them bear identical names.

But there is something besides. Professor Kroeber, in order to

make his point, relies on purely qualitative data, the presence of clans bearing identical names throughout the entire Pueblo area. It is, however, vital to the discussion to see in what relative numerical proportions these clans occur. Unfortunately we can not do this for all of the Pueblo towns but we can do it in the case of a few of them, the Zuñi, the Hopi pueblos, and Cochiti. On page 149 Professor Kroeber has himself given a numerical comparison of the principal clans in these towns, the true figures being altered just sufficiently to yield a fair comparison between them. He has not, however, separated the two Hopi pueblos of the east Mesa, Walpi and Sichumovi, which it is important to keep apart, because, according to Hopi tradition, Sichumovi was a Zuñi colony. In the subjoined table I have made this alteration and I have made only three entries, one for each of the groups of clans supposed to have come to Hopi from the north, south, and east respectively. I have used the figures upon which Kroeber depends, merely increasing them proportionately the better to make a direct comparison. I have put all of the Hopi pueblos in one column except Hano as being of Tewan extraction and Sichumovi.

	Hopi	Sichumovi	Hano	Zuñi	Cochiti
Clans from North.....	61	—	—	20	—
“ “ South.....	507	616	892	340	303
“ “ East.....	1068	1022	747	1280	1336
	<hr/> 1636	<hr/> 1638	<hr/> 1639	<hr/> 1640	<hr/> 1639

If there is anything in the traditions under consideration we ought to find those clans from the north best represented comparatively in Hopi, most poorly represented in Cochiti, the farthest east of these pueblos, and poorly represented in Hano because it was a pueblo of eastern origin and in Sichumovi because it is supposed to have come from Zuñi. And in fact it is best represented among the true Hopi and not represented at all in the other pueblos mentioned. It is true that there are a few of this group at Zuñi but as between Zuñi and Cochiti this is what was to have been expected. It is true that we ought also to have expected better representation at Hano and Sichumovi; a hundred per cent. confirmation is not, however, to be looked for. Again, as between the clans from the south and east we ought to expect the representation of southern clans greatest among the Hopi and poorest at Cochiti, and—as between the Hopi, Zuñi and Cochiti—this is precisely the case. Why southern clans appear to be represented better still at

Sichumovi and Hano I do not know, but the populations of these two towns are so small that a slight increase or decrease on one side would result in upsetting the proportions very considerably.

I do not consider these facts to have disposed of this question by any means. If we could have a clan census from every other pueblo it is likely that a very different condition of affairs would be disclosed. Yet, admitting the whole question to be provisional, I feel as much justified in accepting Hopi tradition as embodying a true kernel of fact as Professor Kroeber in rejecting it because clans are qualitatively present throughout the Pueblo area. All of this shows that we need much more information from the remaining pueblos, and more studies of the kind here under consideration.

JOHN R. SWANTON.

Zuñi Kin and Clan. A. L. KROEBER. (Anthropological Papers of the American Museum of Natural History, vol. XVIII, pt. II, pp. 39-204, 3 text figures and 9 maps.) New York, 1917.

Dr. Kroeber in his admirable monograph on "Zuñi Kin and Clan" throws down the gauntlet to the partisans of the theory of the clan as the basis of society. The challenge at large is no doubt called for, and to Dr. Kroeber's appreciation of the importance of the blood kin compared with the clan and of the subordination of clan to tribe I take no exception; nevertheless I incline to think that Dr. Kroeber has misinterpreted or undervalued in theory, even on his own scrupulous array of data, the position of the clan in Zuñi. He has carried through this theoretical undervaluation along three lines, first by separating too rigidly the ceremonial functions in Zuñi culture from what he calls social functions, and limiting clan participation to the former; second, by overlooking, in comparison with his otherwise careful research, a number of economic as well as of ceremonial facts which may have considerable significance in clanship analysis; third, by leaving out of consideration the more or less conscious attitude of the Zuñi themselves towards clanship. That a woman should be reprimanded by her husband for having spoken bitterly of a clansman, reprimanded merely because the man was her clansman, an incident once described to me, may be potentially as significant a fact as the fact of one national criticizing another for abusing a fellow countryman. Given such a sense either of nationality or of clanship it can hardly be said that "the clan [or the nation] is not thought of in ordinary personal relations of man to man, or of man to woman" (I fail to see how the fact of clan exogamy does

not enter daily into the personal relationships of man to woman), and only by taking into account the psychology of the clan sense of solidarity could such extraordinary facts as migration by clan be understood. Would Dr. Kroeber say that he observed at Zuñi no expressions of clan solidarity sufficient to account for any such migration by households unrelated by blood as are reputed to have occurred for example from Zuñi to the Hopi or from Laguna to Isleta? I realize that it has still to be proved that such households were unrelated by blood or even that clanship was a selective principle among the immigrants.

Zuñi clans play an important part, as Dr. Kroeber admits, in Zuñi ceremonialism, but if ceremonialism is itself, as it undeniably is at Zuñi, an essential of both public and private economy, how can it be said that clanship is "only an ornamental excrescence upon Zuñi society"? To say that without the clans the forms of Zuñi religion will have to be made over in part, "but the life and work of day to day, the contact of person with person, will go on unaltered," is to the student of Zuñi ceremonialism much like saying that however men express thought or feeling, they breathe and digest, they are born and they die.

A more persuasive content would have attached to Dr. Kroeber's thesis in regard to the clanlessness of daily life had he shown in a number of families or even in his one pedigreed family the amount of family intercourse and cooperation which occurs, to what extent related households work together in agriculture—in planting, in harvesting and in threshing, in sheep-raising and dipping, what relations cut feather-sticks for others, or have attended at childbirth or at death or burial, or what relatives have adopted children or given a roof to the discontented at home. All of these economic or ceremonial activities he would have found performed, I have no doubt, by kin and not by clan, but he would have had to consider whether, kin lacking, any of them would have been undertaken by clan. The preparation of a corpse and its burial are undertaken by the paternal kin, but supposing paternal kin are lacking, would not the paternal clanswomen and men of the deceased render the requisite offices rather than his maternal kin or his own children?

And is Dr. Kroeber certain that in planting feather-sticks certain *koko* or gods are not viewed as clan ancestors, or that in the building or repair of the *shalako* houses, in supplying them with wood, and in the field work of the *shalako* hosts, clanspeople of the hosts or impersonators in the ceremonial, mere clanspeople, do not help? In the repair of the house of the ranking priesthood Stevenson states explicitly (*The Zuñi Indians*, p. 227) that the work was done by the clanspeople of the priests,

the workmen appointed by the *pekwin* or sun priest, a member of the same clan as the aforesaid priests. The names of the appointees were called out by one of the *tenientes* or council. Clearly here was a case of communal labor by clan, ceremonial, if you like, as well as economic; but it may well be a ceremonial survival of an early provision of merely economic clan labor.*

"Clan and kin are distinct things, one rather lightly superimposed upon the other," concludes Dr. Kroeber. We would run less risk of the very schematization Dr. Kroeber criticizes elsewhere, it seems to me, were we to figure the relation between kin and clan as a circle with the kin at the center and the clan at the circumference, letting the radii represent the functions, ceremonial or "social." When these functions can be performed by a few persons the kin, as long as kin are available, are called upon; but when the kin are unavailable or when the services of a comparatively large number are desired clanspeople or the clan as a whole are called upon. Take the clan relation towards the *koyemshi*. The final day of the *shalako* ceremonial the head of each *koyemshi* has to be washed and his back loaded with presents. Members of his father's clan nearest to him in blood perform these functions. But all his father's clansmen have cut feather-sticks for him and male and female representatives from the households of his father's clan march into the plaza bringing him gifts of meal or bread, of melons, of mutton and store-bought goods. Here is one occasion at least, Dr. Kroeber to the contrary, where the clan does act as a whole.

To ensure the clan gifts to the *koyemshi* messengers have gone from house to house. These messengers belong to the household of the *koyemshi's* father's sister, his *kuku* par excellence, I think, I am not sure. The alternative is that the messengers belong to a household having particular clan authority or leadership. Dr. Kroeber would summarily dismiss this alternative with the statement that there are no clan heads. I have heard too many references to the selection of ceremonial impersonators by clan heads to be satisfied with Dr. Kroeber's conclusion in general on this point without further investigation. Indeed the whole subject of the selection of persons for ceremonial rôles belonging to or associated with clans needs the most careful study. Take the present confusion of our knowledge about the impersonation of *pautiwa*, *koko awan mosi*, the gods their director. Stevenson states that *pautiwa* is chosen by the Corn clan and the children of the Corn clan (persons whose fathers are Corn clansmen) assembled in the house of the head of the clan. Dr. Kroeber states that *pautiwa* must be a Dogwood clans-

man and selection by the clan as a body he disputes as unparalleled in Zuñi practice. According to my informants *pautiwa itiwonna* (*pautiwa* of the winter solstice ceremonial) must be a Dogwood although the two men of whom one or the other now habitually impersonates *pautiwa* were sons of a Turkey clansman who in his day impersonated *pautiwa* and who *handed the position down to his sons*. This Turkey clansman was, however, child of the Dogwood. Unfortunately my notes leave a doubt as to whether the present impersonators are Dogwood. If they are their fathers must have taken the rather unusual step of marrying into his father's clan. In spite of these facts my informants stated that the impersonator of *pautiwa* would be selected by the *pichikwe amosi*, the directors or heads of the Dogwood people.—But *pautiwa itiwonna* is not the only impersonation of *pautiwa*, there is *pautiwa molawia* (the impersonation in the *molawia* ceremony at the close of *shalako*). The impersonator of *pautiwa molawia* is or should be a Tobacco clansman selected by the heads of the Tansy-mustard clan, by the same "heads," I surmise, who choose the girls who run in the *molawia*.—Rather complicated!

Another point in regard to clan heads. Is it not possible that the clan *ettowe* or fetiches listed by Dr. Kroeber convey a certain distinction or headship to the families in charge of them if merely in accordance with the conceptual schematization Dr. Kroeber points out so well as characteristic of the Zuñi? The argument that the possession of *ettowe* by the clans, however, is evidence "that the Zuñi clan is much more a part of a ritualistic scheme than a body of kindred" is to my mind not valid, for may not the *ettone* be merely a pattern which is associated with group formation, with the formation of any group? Incidentally I would suggest that the *ettone* is property, quite as much property as the "good will" of an American business or the saint's image in a Catholic church (I surmise that the Zuñi think of their *santu* as an *ettone*. As for the canes of the *tenientes* I venture to say that they too are approximations of *ettowe*), and in so far as the clans are possessed of *ettowe* it can hardly be said that there is nothing "that can be considered clan property."

It is from lack of data that Dr. Kroeber disregards the possible significance of the clan *ettowe*. His disregard of the question of leadership among the clans is more wilful. In view of the fact that the ranking priests of Zuñi must belong to the Dogwood clan (likewise as I have said, the personator of the ranking figure among the *koko* or *kachina*), it is difficult not to recognize a degree of clan supremacy and not merely

clan equivalence at present in Zuñi or in view of the clan autocracy at Acoma, for example, not to speculate as to a still greater degree of clan preponderance at Zuñi in the past. Take the matter of communal service for the ranking priesthood. In Acoma four sacred rabbit hunts are held yearly and the game given to the paramount priest; his land is also planted for him by the people and it is he or his "uncles" or "brothers" who assign farm lands to the people. Nowadays in Zuñi only one sacred rabbit hunt is held every four years (the game goes to the ranking priesthood) and there is neither planting of priest-owned fields by the people nor land allotment by the priests, but may not the priestly prerogatives and the priestly control have once been larger—before Spanish interference? At least the question needs investigation.¹

Most painstaking and accurate are Dr. Kroeber's data in kinship terminology but here again because of his clan prejudice his comment or inference is sometimes open to question. Is not the Zuñi indifferent to fine discriminations of consanguinity and uninsistent upon carrying relationship back very far not because he is merely linguistically contradictory or merely uninterested in genealogy but because of his clan consciousness? It is just as you might answer when asked of one John Smith, a seventh or eleventh cousin, "Oh I don't know exactly how he is related, but he is one of that Smith family." Given the closed circle of clanship, fine distinctions are not urgent, only gross distinctions, such as my own mother, my own father, my blood relatives, are in order.

Towards the evidence kinship terminology may have for relationships not found today at Zuñi it seems to me that Dr. Kroeber is somewhat impatient. The similarity of terms for a woman's brother's son and for son-in-law, is, as Dr. Kroeber says, a striking argument for cross-cousin marriage nor is Dr. Kroeber's preferred explanation incompatible, as far as I can see, with the argument. Cross-cousin marriage certainly does not exist today in Zuñi nor does marriage with a deceased brother's wife and yet I would not close the case for cross-cousin marriage in the past or, if the Zuñi call a father's brother's wife *inniha*, stepmother (that so careful an observer as Dr. Kroeber did not get this application of the stepmother term *inniha* would make me doubt

¹ Judging from a recent land dispute, sacerdotal land control would not seem foreign even today to the Zuñi point of view. Kwanton, an ex-governor, holds a Spanish deed to land to which in 1916, if not before, he was setting up a claim. The *tenientes* refused to consider the claim, the lieutenant-governor throwing down his cane in the plaza in the course of the quarrel. As a result of this controversy as well as of other matters in dispute—the *tenientes* were changed by the ranking priests and Kwanton's party, as we would say, recognized.

the accuracy of my informants except that Dr. Kroeber himself found some puzzling applications of the term), for the levirate merely because the Zuñi themselves repudiate and scorn the idea of the practices. The Zuñi are not only thorough believers, as Dr. Kroeber has pointed out, that whatever is, is right, but that whatever is not, is wrong. In suspending conclusions on early marriage customs, I might even wonder what Zuñi women mean when they refer to *shalako* night as a time "for stealing a word with our other husbands" were I not afraid of being derided by Dr. Kroeber as one fascinated by the theory of promiscuity.

Nevertheless I have courage enough to hold that "the marked tendency towards descent from father to son in the priesthood" is of interest—both psychologically and historically. Dr. Kroeber suggests an interesting psychological basis for the clan, namely that a woman is *felt* to be a very different thing from a man in relationship. Would he not be willing to entertain the hypothesis that paternal descent becomes a principle in ceremonialism because a man is felt to be a very different thing from a woman in religion? From the historical standpoint, that patrilinear kinship tends to be associated with Zuñi ceremonialism may not be an unimportant fact at all. Suppose, for example, that it becomes established that part of the complexity of Zuñi ceremonialism is due to the incorporation of Catholic rituals, would not the presence of the principle of paternal descent in a ceremonial be at least one clue as to whether in other particulars Spanish acculturation was to be looked for?

As a parting shot on the interpretation of Zuñi clanship let me ask Dr. Kroeber why if it is true that to the native mind the clan is "essentially a schematic subdivision, and perhaps a more or less artificial one, of the community as a whole," why if it is true that the clan is "a ceremonial rather than a socially functioning body" (a misleading distinction as I have said before), then why are the Zuñi so conspicuously ready to talk, as Dr. Kroeber truly remarks, about their clan affiliations, reticent as they are about all ceremonial matters (notice that about blood kinship they are not reticent either) and why, seeing that a Zuñi will change from fraternity to fraternity and even from *kiwilsine* to *kiwilsine*, and that he may drop out of his priesthood, why does he never change from one clan into another or ever dream of dropping out of the clan he was born into? A Zuñi may be taken at any time of life into a household other than that he was born to, adopting the kinship terms and attitudes proper to his status in the adoptive household, and a Zuñi may go to live in another tribe. How deeply he may feel the effects

of either the tribal or the household transplantation we do not know, but one thing we do know, neither tribal nor household adoption has any effect upon clanship. How does clanship alike in native theory and practice come to be the most *fixed* character of the whole social organization?

ELSIE CLEWS PARSONS

ASIA

The Foote Collection of Indian Prehistoric and Protohistoric Antiquities. Madras Government Museum. Catalogue raisonné. ROBERT BRUCE FOOTE. Government Press: Madras, 1914. 262 pp.

The author of this catalogue, Mr. Foote, who died in 1912 at the age of seventy-eight, was a geologist, who joined the Geological Survey of India in 1858 and retired as senior superintendent of the service thirty-three years later. The greater part of his service was spent in southern India, where he accomplished much in researches of the geology and palaeontology of the country. In 1863 he succeeded in discovering palaeolithic implements near Madras, and thus became the pioneer of prehistoric archaeology in that region and subsequently a recognized authority. His extensive and prolonged journeyings in southern India enabled him to accumulate an extensive collection of objects found chiefly in the Madras Presidency and the neighboring states of Mysore and Hyderabad, also in Baroda and other parts of northern India. The entire collection was purchased by the Madras government in 1904, and a special room was built for its reception in the Art and Ethnological Section of the Madras Museum. The author did not live to see his catalogue out, but had revised most of the proofs. The work, as stated in the preface, has been brought out in two volumes, the second to contain notes on the ages and distribution of the antiquities, plates, map, and index. This second volume has not been sent to me for review or is not yet out. The present first volume (not thus designated on the title-page) contains only a descriptive list of the objects in the collection, arranged according to the districts and localities in which they were found. Altogether 4135 specimens are listed, in addition 68 from Ceylon, 205 beads, and 128 bangles and glass frits, these groups being numbered separately. The descriptions are the briefest possible, only form, material, color, and locality being noted. With the exception of a few cases, no information is given regarding the circumstances of the finds, which would be of especial importance with reference to the glass objects, as the time for the introduction of glass into India has not yet

been determined. Measurements are not added. We hope to come back to this work when the second volume is available.

B. LAUFER

Catalogue of the Prehistoric Antiquities from Ādichanallūr and Perumbāir. Madras Government Museum. ALEXANDER REA. Madras, 1915. 50 pp. 13 plates in half-tone.

This catalogue offers a description of the important finds made in the burial ground at Ādichanallūr (the southernmost district of the Madras Presidency), covering an area of one hundred and fourteen acres and the most extensive yet discovered in southern India. The excavations were conducted by the author of the catalogue at intervals from 1899 to 1905. These graves are characterized by funeral urns deposited in pits, which were excavated in the solid rock or in the gravelly soil. On the whole, they are similar to those found in other graves of southern India. The pottery is unglazed, but exhibits a red and black polish, and almost lacks decoration. Of greater interest are the objects of metal; chiefly iron, bronze, and gold. The only gold objects are oval diadems of a thin, flimsy gold leaf, supposed to have been tied around the forehead of people of rank and to be substitutes of a more substantial piece of jewelry, as worn in life. Of iron, many implements and weapons like swords, daggers, tridents, hatchets, spearheads, and arrowheads, were found, always placed point downward, as if they had been thrust into the surrounding earth by the attendant mourners. No implements or weapons are made of bronze, all articles of this alloy being vessels of various shapes, personal ornaments, such as rings, bangles, and bracelets, or ornaments attached to the bases and covers of vases, such as buffalo with curved horns. Iron, then, served for the implements of everyday life; bronze was the material for artistic expression. The domestic animals represented in bronze in full figures are buffalo, dog, sheep, and rooster; the wild animals are tiger, antelope, and elephant. The animals are usually arranged in circular rows on a complicated metal framework. Although of crude workmanship, they can be readily identified. A dog is represented with a great deal of realism and motion. The cow and other animals distinctive of Indo-Aryan culture are absent. The bronze alloy consists of copper 75 per cent., tin 23 per cent., lead 0.2 per cent., iron 0.4 per cent. The bronzes, it is said, exhibit a high degree of technical skill and manipulation of the metal. On several of the bronzes are traces of textiles, preserved by contact with the oxidized metal. Many of the vessels contained rice and millet seeds. In a

number of urns there were fragments of mica in pieces about an inch in length.

The second and smaller collection included in this catalogue bears on prehistoric remains from the neighborhood of Perumbāir, about fifty miles south of Madras, excavated by Mr. Rea from 1904 to 1908. Here the ancient burial sites are indicated on the surface by circles of rough stone boulders; and in the center of each circle, at a depth of from two to seven feet, was found either a pyriform urn or an earthenware cist, covered with a dome-shaped lid, and posed on three rows of short legs. In and near the graves were found pottery, stone implements for grinding, a few iron objects, and some chank-shell ornaments.

The objects illustrated on the thirteen plates are reproduced on too small a scale (on one plate as many as forty-one pieces of pottery are arranged), and do not allow the study of details. Measurements are given in inches.

B. LAUFER

The Mythology of All Races. Vol. VI: *Indian.* A. B. KEITH. *Iranian.* A. J. CARNOY. Marshall Jones Co.: Boston, 1917. Pp. ix, 404, 5 figs., 44 pls. Price \$6.00.

Keith and Carnoy are to be heartily congratulated on these first attempts at a historical and synthetic treatment of Indian and Iranian mythology. Carnoy's account is the first of its kind. For Indian mythology there are a few adequate works on special subjects such as Macdonell's *Vedic Mythology*, Hopkins' *Epic Mythology*, Grünwedel's *Mythologie des Buddhismus in Thibet und der Mongolei* and *Buddhistische Kunst in Indien* (translated by Burgess); but there is no general historical treatment. Both accounts adequately fulfill their purpose as popular summaries; both will also be of value to specialists. Keith's judgment is sober and cautious; Carnoy is more speculative and venturesome in the projection of facts into theories.

Keith devotes two chapters to the Rig-Veda, one to the Brāhmaṇas, two to the Epic, one to the Purāṇas, one to Buddhism, one to Jainism, and one to Modern Hinduism. Carnoy divides his material into the treatment of the wars of gods and demons, of myths of creation, of the primeval heroes, of legends of Yima, of traditions of the kings and Zoroaster, of the life to come.

Keith keeps strictly to the main line of mythology and offers little on the development of myth to legend, folklore, and traditional history. Carnoy devotes much space to the latter development in the Persian

epic. In India the mythological material is so vast, the historical background so obscure, and so little of the preliminary work of analysis and classification has been done that no well-rounded treatment of the subject as a whole is possible at present. Much of the material of the so-called Indian mythology is Dravidian rather than Aryan, but it is impossible at present to distinguish Aryan from Dravidian with any certainty.

The most satisfactory chapters of Keith are those on the Rig-Veda, the Brāhmaṇas, and the Epic. Here he is completely master of his material. The mythology of the Purāṇas, of Modern Hinduism, and of Jainism is sketched cursorily in only the broadest and most general outlines. The chapter on Buddhism is the least satisfactory of all. Here Keith's touch is not so firm and sure as in the earlier chapters. Keith argues (p. 188) that "we have no assurance that a single Buddhist text which has come down to us is even as early as two hundred years after Gotama had departed." So much may be granted, but if this assumption is made it is not fair to claim that we have "the authority of the Buddha himself for his abode in the Tuṣita heaven and his descent from it" (p. 194) or to speak of "the Buddha's belief in his own super-human nature" (p. 193) and "the natural explanation that the Buddha, like his followers, regarded himself as really divine" (p. 195). In India myths and legends develop with amazing rapidity, and two hundred years are not to be dismissed lightly. If no text can be assigned with certainty to the period within two hundred years after Buddha's death there is no certainty that the important mythological matters ascribed in the Pali texts to Buddha himself can be earlier than two hundred years after his death. This is admitted by Keith himself on p. 188: "for we have not, and never can expect to have, any conclusive proof as to the actual views and teachings of Gotama." The contention on p. 189 that "it was clearly more easy for a preacher of faith in a personal god to become regarded as himself a god than to deify a man who *ex hypothesi* was no god and had no real belief in the gods" may be true of the Anglo-Saxon mind, but is, I think, utterly fallacious in the case of the Hindu mind. In the dispute between those who approach Buddhism from the point of view of the Hīnayāna and the Mahāyāna texts respectively one thing is certain, that the Pali texts represent the ideas of only one Buddhist sect. Different groups of monks interpreted the enigmatical teachings of the Buddha according to their own thoughts and feelings. The Pali canon does not give us the unified tradition of Buddhism before the early split into sects, although it preserves much

that is at variance with the professed doctrines of the Cinghalese school. Much that is put into the mouth of the Buddha may be due to speculative accretion generations after his death. The "thus I have heard" is no more proof of originality than is the corresponding formula of the Mahāyāna texts. Further, many elements in the life of the Buddha himself (if any of the traditions concerning his life and deeds go back to real memories of his followers) show that he himself lived a life of ministry nearer in many ways to the Bodhisattva ideal of the Mahāyāna than to the Arhat ideal of the Hīnayāna.

Both Keith and Carnoy (pp. 5, 25, 30, 263) refer to the names A-ru-na-aš-ši-il (or U-ru-w-na-aš-ši-el), In-dar (or In-da-ra), Mi-it-ra-aš-ši-il, and Na-ša-at-ti-ia-an-na found in the Hittite records at Boghazköi as Indo-Iranian. The reviewer has tried to show in a recent article in *The American Journal of Semitic Languages and Literatures*, 1917, pp. 261-82, that the matters involved are, as yet, purely linguistic ones on which no historical conclusions should be based.

Keith argues here (pp. 125 ff.), as in previous articles, that Krishna was originally a vegetation-god, become anthropomorphic. He is over-skeptical of the possibility of men becoming gods in India. The classical prejudice against Euhemerism is not valid for India. In the case of Krishna it is much easier to explain the mythology as a later accretion (as in the cases of Buddha and Mahāvīra) than as fundamental. The reviewer can interpret the Krishna of the epic only as a man deified. Where among the vague anthropomorphic figures of Indian mythology is there one which has become so concretely human as the Krishna of the epic? The Krishna (son of Devakī) of the Chāndogya Upanishad 3, 17, 6 cannot be lightly dismissed as not identical with Krishna (son of Devakī) of the epic just because the identification invalidates a theory that Krishna must be a sun-god or a vegetation-god. The passage of Patañjali (p. 126 and *Zeitschrift der deutschen morgenländischen Gesellschaft* 64, p. 536) is far too late to prove that the mythological elements were original. Keith argues (*Journal of Royal Asiatic Society* 1915, p. 547) that a mere man is not sufficient to explain the mythological elements of the Krishna story. It may be answered that a mere god is not enough to explain the concrete human figure of the epic Krishna. On p. 126 Keith does not express definitely his attitude toward Garbe's very doubtful identification (*Indien und das Christentum*, pp. 215 ff. and *Archiv für Religionswissenschaft*, 1913, p. 537) of Buddhist Rummidei (Lumbinī) with Rukminī, the name of Krishna's wife. There is no convincing proof that Krishna worship had penetrated so far east-

ward by the sixth century B.C. For philological difficulties in the way of the identification see Charpentier (*Indian Antiquary*, 1914, p. 18).

Keith (p. 177) is inclined to follow Garbe's theory that the Çvetad-vīpa story is to be explained on the basis of Hindu contact with Nestorian communities in the vicinity of the Balkash Sea. Garbe (in *Indien und das Christentum*) has summed up the weighty evidence against Weber's theory that the story is to be explained as referring to Christian communities at Alexandria; Pelliot (*T'oung Pao* 1914, pp. 623-44. Cf. Laufer in *American Anthropologist*, 1916, pp. 572-3) has made available material which utterly invalidates Garbe's theory, at least so far as present evidence goes. There remains only the conclusion that the story is mythical (see Charpentier, *Journal Asiatique*, 1910, II, p. 605). It is most unlikely that any early Christian community could have served as the basis of such a description. The story may belong to the same development as that which resulted in the Buddhist descriptions of Sukhāvātī. It is more probable that the word *dvīpa* of Çvetad-vīpa is to be connected with the mythical *dvīpas* of Hindu cosmology than with any actual island.

In connection with the story of the Bhaviṣya Purāṇa (p. 183) concerning the "Magas" Keith might have referred to the important collection of material made by Spooner ("The Zoroastrian Period of Indian History" in *Journal of Royal Asiatic Society*, 1915, pp. 63 ff., 405 ff.). Spooner's theories are far more pretentious and wide-reaching than the facts warrant, but the article should stimulate a closer consideration of the possibilities of early intercourse between Persia and India. Jackson's "Notes on Allusions to Ancient India in Pahlavī Literature and Firdausi's Shāh Nāmah" (in *Festschrift Windisch*, p. 209) seems to have escaped notice in the bibliographies of both Keith and Carnoy.

It is by no means certain that the starting point for the personification of Vāc was the thunder (p. 53). That is one of Yāska's guesses not fully corroborated by the evidence of the Rīg-Veda itself. Vāc is probably the personification of the voice (spoken word or hymn) which, as the sacrifice assumed a cosmic aspect became cosmic too.

The bibliography, though well chosen, is, of course, far from complete. I would add at least such titles as Dowson, *A Classical Dictionary of Hindu Mythology* and Garrett, *A Classical Dictionary of India*—helpful, if not useful for scholarly purposes; O. E. Martin, *The Gods of India* (Dent, London, 1914)—an uncritical but very useful popular account; Vans Kennedy, *Researches into the Nature and Affinity of Ancient Hindu Mythology*—old, but giving many translations from the

Purāṇas not to be found elsewhere; Coomaraswamy, *Buddha and the Gospel of Buddha*; H. K. Sastri, *South Indian Images of Gods and Goddesses*; H. Oldenberg, *Die Lehre der Upanishaden und die Anfänge des Buddhismus*—a work of fundamental importance for the social and intellectual background; G. Oppert, *Die Gottheiten der Inder in Zeitschrift für Ethnologie*, 1907, pp. 296, 501, 717. Lists are given of all the articles in the first eight volumes of Hastings, *Encyclopædia of Religion and Ethics*, bearing on Indian and Iranian mythology.

Keith maintains a wise reserve on the question of the early relations between Buddhism and Christianity. *Sub judice lis est*.

The Simurgh story as related by Carnoy (p. 289) has striking parallels with the story of Garuḍa in Mahābhārata I, 29, 30.

Carnoy's description of Narācaṃsa as the sacrificial fire in India (p. 284) and references to pages 44-45 is not borne out by the statement of Keith, who refers to Narācaṃsa as "the personification of the praise of men, or possibly the flame of the southern of the three fires, which is particularly connected with the fathers." Narācaṃsa is a very uncertain figure. There are several different theories concerning him.

Carnoy has done a great service in calling attention to many curious coincidences between Iranian myths on the one hand and Indian and Babylonian myths on the other. No definite conclusions can be drawn, as yet, from what may be mere coincidences, but as the historical evidence becomes fuller such collections of parallels will be valuable. He deserves high praise for the success of this first effort to treat Iranian mythology historically and to bring Avestan mythology into connection with the legends of the later Persian epic.

WALTER EUGENE CLARK

SOME NEW PUBLICATIONS

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Museum of Natural History, vol. xxii, part i, pp. 1-73, 18 text figures.) New York, 1917. Price \$.75.

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——. One of Caesar's Anecdotes among the Indians of Northeastern North America. Alumni Register, University of Pennsylvania. pp. 686-690. June, 1916.

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Wissler, Clark. Notes on the Social Organization and Customs of the Mandan Hidatsa and Crow Indians. (Anthropological Papers of the American Museum of Natural History, vol. xxi, pt. i. Pp. 1-99. New York, 1917. Price \$1.00.

DISCUSSION AND CORRESPONDENCE.

MOCCASINS

IN the April-June number of the *American Anthropologist* (vol. 19, pp. 297-301), Dr. B. Laufer has made some friendly and critical remarks on my paper *Moccasins and their Relation to Arctic Footwear*. I am grateful to Dr. Laufer for the interest he has taken in my work; I find it necessary, however, to make a reply to his criticism.

Dr. Laufer regrets that I do not grasp the origin and culture-historical position of other types of footwear than moccasins. He is not quite right in this statement. Among the best results yielded by my study of clothing I reckon the analysis of the Eskimo boot (*Arktiske Skinddragter*, pp. 179-194, recapitulated in *Moccasins, etc.*, pp. 201 f.) and the tracing of its evolution from a sandal and a stocking. I have been able also to prove the existence of "sandal-boots" in different parts of northern Asia and in Pamir, and my material has led me to suggest that "sandal-boots" have a wide distribution in Central Asia (*Arktiske Skinddragter*, pp. 194-96, *Moccasins, etc.*, p. 234 note and p. 236). I have not, however, made Central Asiatic footwear the special subject of any publication. In my moccasin paper I have only touched the question of Central Asiatic footwear briefly, just to give some reasons for my view, that true moccasin-forms were not characteristic of, perhaps never found in the clothing of Central Asiatic nations. I found it unnecessary to enter further upon the study of Central Asiatic footwear in my moccasin paper. It is gratifying to me that Dr. Laufer supports my view regarding the absence of connection between the moccasin-group and the footwear of Central Asia; he even states that the latter has "positively nothing to do with the moccasin." So much the better for my theory about an original connection between the moccasin and the snowshoe.

I think Dr. Laufer is right when he assumes that the riding-horse has a share in the evolution of the riding-boot; I do not see, however, that this theory affords any full evolutionary history of the "riding-boot"—it says nothing at all about the prototype of the "riding-boot." It even remains to be seen, whether all riding-boots have sprung from one prototype—this can only be proven by means of a thorough analysis of all riding-boots extant. I must confess, though, that I am rather

surprised at Dr. Laufer's disapproval of my "transformed moccasins" in the Amur country and Siberia. My "transformed moccasins" are not hypothetical—they are in existence; a full series of "moccasin-boots," illustrating the transformation of the moccasin into a boot, is given by me (*cf.* fig. 24, 59 and 71-84 in *Moccasins*). The question remains, of course, *how large* a share the moccasin has had in the evolution of the footwear of northern Asia. When the transformed moccasin at last loses all its moccasin-traits, we lose track of it. I have never asserted that *all* boots of northern Asia were moccasin-boots—on the contrary, I have proven that there are boot-forms in northern Asia which have nothing to do with the moccasin, but are evolved directly from the sandal and the stocking.

Dr. Laufer regrets that I have not cited the *Publications of the Jesup North Pacific Expedition*, Leopold v. Schrenck's work on the Amur peoples, nor the Russian literature. The reason for this is quite simple. I did use the well-known publications of the Jesup expedition and of v. Schrenck in my book on arctic clothing, and also the Russian literature, as far as it was available, was used and cited by me in that work. I have not, however, found anything in these very valuable publications that was of any use to me in my moccasin paper, therefore I did not cite them there. Of course, they contain descriptions and illustrations of footwear, but no information about moccasins or "transformed moccasins" which was not to be had in fuller and better detail by studying the collections in museums. I have not found it expedient to cite unnecessarily all the authors that have described moccasin-forms; only when I have learned something from them on this particular subject, have I mentioned their publications. Nor have I cited the immense number of more or less superficial and occasional descriptions of American moccasins, which I have come across in my search of information.

On the last two pages of my moccasin paper, I have ventured to set forth a very far-reaching hypothesis which—it may be justly said—is as much out of proportion to the study of moccasins as a church-steeple to a hut. I was much in doubt whether I should not abstain from such short and extremely insufficient mentioning of my "inland and coastal culture" theory; it was beyond my power, however, to suppress altogether my "working hypothesis," the underlying idea which has made even boots and old pelts interesting to me. By tracing my hypothesis in such short outline, I have, however, invited misunderstanding. My theory is not, as Dr. Laufer thinks, based upon the geographical dis-

tribution of one single ethnographical phenomenon—it is supported not only by the data of clothing and of skin-dressing, but also (as suggested on p. 249 of my moccasin paper) by the distribution of different types of habitation, means of transportation, modes of hunting, reindeer-nomadism, etc. In fact, the theory rests upon the geographical distribution of the types of material culture in the northern regions, and would require for its elucidation a full and detailed analysis of all the different sides of the material culture of northern tribes. The preparation of such a full presentment of my theory demands, however, more work than can be done in a short while.

When I find the "inland culture" fullest developed and least mixed with "coastal culture" among the Tungusians of Siberia, I do not intend to say that the Tungusians were the first originators of the "inland culture." Nor do I hold the opinion that the oldest "inland culture" was identical with the Tungusian culture of the present day. The Tungusian area contains the highest development of the "inland culture," whose older aspects are represented outside of this area; for this reason I hold the opinion that the geographic area, now inhabited by the Tungusians, has been from the outset of great importance for the development of "inland culture." A chief cause of this probably was an influence of cultural stimulants from the south.¹ On the whole, it is not among the Tungusians that we must look for the oldest traits of the "inland culture"—the evolution of "inland culture" has been most rapid there and has effaced the old traits. For this very reason, however, I find it probable that the area, now inhabited by Tungusians, contained the fountain-head of the "inland culture," perhaps long before the Tungusians entered Siberia.

I do not at all presume "that the ethnical conditions of northern Asia were the same anciently as at present." Such an assumption would involve on my part a gross and unpardonable ignorance of ethnological literature. I find it, however, extremely useful to study the geographic

¹ As mentioned on p. 249 of *Moccasins, etc.*, I find in reindeer-nomadism certain elements due to influence from more southern forms of nomadism. These southern elements are nowhere as apparent as among the Tungusian reindeer-nomads. The use of the reindeer for riding certainly represents a loan from the nomadism of Central Asia and probably is younger than the use of the reindeer as a draught-animal; the types of harness used in driving with reindeer are variations of the same fundamental type everywhere from the Lapp to the Chuckchee (this "barbaric" spelling I have adopted from the *Publications of the Jesup Expedition*), and there is a very significant similarity between this type and the forms of dog-harness used by the Gilyak and by the old Kamchadal. The methods employed in herding the reindeer, however, contain some elements which have their root in the reindeer-hunt.

distribution of ethnographical phenomena, not only on the American continent, but on the Asiatic as well. Historic sources should, of course, be consulted whenever possible; they are, however, never sufficient when we want to reconstruct the culture of bygone ages—not in Europe, still less in northern Asia. As to moccasins and snowshoes, a theory about their origin and spread must always be founded upon a study of the actual forms and their geographical distribution; this is what I have tried to do. I have not attempted to “reconstruct a history” of the peoples of northern Asia.

I am quite aware that my assumption of a Siberian origin of the Lappish footwear is somewhat strengthened by the fact that a considerable part of the Lapp culture is closely related not only to that of the Samoyed, but also to that of several other Siberian tribes. It is hardly possible to study the cultures of northern Europe and Asia for years without recognizing this fact. It would, however, have been ridiculously out of place, if I had drawn the question of the origin and history of the entire Lapp culture into the discussion of moccasin-forms—this question is so intricate and many-sided, that it would require very considerable space.

GUDMUND HATT

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Dr. Laufer has called attention to some inconsistencies and infelicities in the spelling of the proper names in Dr. Hatt's paper. The editor is solely responsible for these, for when a choice of spelling is permitted the editor makes the choice that there may be a certain amount of uniformity and consistency running through the various volumes. The retention of F. C. M. to refer to the institution now rechristened Field Museum of Natural History is also the fault of the editor, who hesitated to make the change when some of the very numerous occurrences of the abbreviation in the manuscript were almost sure to be overlooked.—Ed.

THE TWO BELIEFS IN PERSONAL IMMORTALITY

THE scientific writer on religious subjects is often discussed by incompetent or prejudiced persons. When, as in the case of the review of my book¹ by Professor Ogburn in the last issue of this journal, the reviewer is both competent and dispassionate the writer may well think himself fortunate.

Professor Ogburn wishes that I had published more fully the tables upon which my statistical conclusions are based. It was my desire to do so, but after bearing the expense of the investigation itself, I shrank

¹ *The Belief in God and in Immortality. A Psychological, Anthropological, and Statistical Study.* Sherman, French and Co.: Boston, 1916. Pp. xvii + 340.

from increasing the printer's bill by the publication of tables that after all were not necessary.

I cannot quite accept the criticism referring to Part I (the Two Conceptions of Immortality, their Origins, etc.). Professor Ogburn accepts in general the distinction I established between the primary and the modern conceptions of immortality, but it seems to him that I "neglect the importance of desire among primitive peoples," and that I magnify the difference between the primary idea and those of the moderns." Stripped of all non-essentials, what I set forth was that certain facts to which the savage ascribes external validity, such as visions in sleep, fever, etc., convince him of survival after death; and that a belief arising in that way, *i. e.*, based on what is considered by the savage as actual perception is, in principle, independent of desire. That belief I have called "primary." Whether the savage likes it or not, ghosts are there; he sees, hears, and even touches them. The belief is *imposed upon him* just as that in the existence of the natural objects with which he is surrounded. Such a belief has no moral significance in the sense of reward or punishment for conduct on earth.

Quite different in origin, nature, and function is the modern belief. It originated not in the class of facts which produced the primary belief, but in desire for the realization of ideals of happiness and self-realization. A conception of immortality arising in this manner could not fail to possess the characteristics desired by man and, thus, to become a source of encouragement and inspiration.

Now, it is a matter of record that in many tribes the conception of continuation after death bears little or no trace of having been elaborated into a paradise under the influence of desires imperfectly gratified in this life. These tribes hold the primary belief in its pure form. Its characteristics are adequately accounted for when the origin I have ascribed to the primary belief is admitted. I have not failed to recognize, however, that in other tribes the conception of immortality bears marks of greater complexity than that belonging to the primary belief: it assumes the aspect of a paradise. In the first chapter of my book, under the subtitle "The Primary Paradise" (pp. 15-23), I described, briefly it is true but quite definitely, the presence among savages of desires acting upon the idea of continuation after death. Here we find operating the two classes of factors: perceptual experiences and desires.

But when one passes from these tribes to the higher social level occupied by the peoples bordering the eastern end of the Mediterranean sea, those from which Europe derived its civilization, one finds at the

beginning of the historical period a conception of continuation after death altogether free from the influence of desire. Survival was accepted by those peoples as an unavoidable fact, undesirable, and often to be dreaded. Perhaps my reviewer has not sufficiently considered the demonstration made in chapter III, that among the peoples I have designated the conception of the hereafter was of an undesirable existence to which all were doomed and from which the idea of retribution was as completely absent as that of the gratification of any craving for happiness and self-realization. It is of that belief that I speak when I say that it is radically different from the modern one in point of origin, nature, and function; and that the modern belief is not a continuation of the old but a new creation. The facts are, it seems to me, incontestable; some of them I have related in chapter IV.

The paradisiacal elements that appear in the conception of the hereafter entertained among certain savage tribes, does not disprove the theory of discontinuity between the primary and the modern conception, for these elements had disappeared when the modern belief came into existence.

Why did these paradisiacal elements, born of desire, disappear? Why did not desires assert themselves more and more effectively and gradually transform the primary conception into the modern one? This is a very interesting question toward the solution of which I offered the following suggestion:

The explanation of the temporary triumph of the dismal belief in impotent and vacuous ghosts seem to be found, as I have already intimated, in the inability of men at that stage of culture to conceive of a person as enjoying a tolerable existence when deprived of his earthly body. The persistence of the difficulty offered by the destruction of the body is sufficiently evidenced by the fact that its resurrection is affirmed in the modern conception of immortality (pp. 97-98).

I might add that I do not regard my study of the origin of the beliefs in immortality as complete. My task was a narrowly circumscribed one. The student of social psychology will want to know what are the conditions for the coming into existence of the desires to which I referred the origin of modern immortality. Why did they appear at a particular time, among particular peoples, with an intensity sufficient to break through all impediments to belief? In answer to these questions nothing more than hints are provided in my book.

The problem of origin as I have treated it, comes much nearer being fully solved in the case of the primary belief. For, given a certain

intellectual level and apparitions of deceased persons in dreams and other visions, and the belief seems bound to appear.

JAMES H. LEUBA

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THE DIFFUSION OF CLANS IN NORTH AMERICA

IN a recent article on "Iroquoian Clans and Phratries" (*American Anthropologist*, vol. 19, 1917, pp. 392-402), Mr. C. M. Barbeau advances certain views with reference to the dissemination of clans in North America which to many will appear as both interesting and startling. The keynote of Mr. Barbeau's position is struck in the following paragraph:

The Iroquoian and neighboring Algonkian clans, on the whole, were widely diffused exogamic kinship units, endowed with inconspicuous crest and supernatural guardian systems, the membership of which was inherited in the maternal line or conferred through adoption. As will be seen in the following lists, the clans were few in number; but their component parts were found disseminated in sections of numerous towns of many tribes and nations. Distance and linguistic barriers did not prevent the members of the same clan from acknowledging their relationship and mutually abiding by the rules of exogamy and fraternal help (p. 393).

Having enumerated the clans of the different Iroquoian tribes and indicated their division into phratries, the author remarks:

In the various Algonkian, Siouan, Muskogean, Yuchi, Pueblo, and other lists of clans we find, unevenly represented, most of the Iroquoian clans, besides many others. *The diffusion of the different clans here is in all probability due to direct transmission or to imitation*¹ (p. 395).

Having thus conceived the distribution of clans of the same name as due to "diffusion," the author proceeds to indicate the distribution of the various "clans" among the tribes of North America.

The novel method of identifying historically clans of the same name is the more striking, as the author does not reveal any consciousness on his part of any special problem involved, but seems to regard the interpretation through diffusion as vindicated by the mere facts of distribution. The author takes a similar view regarding the distribution of phratries, expressing his conclusion in the following words:

There is little doubt that, in the course of time, the moiety or phratrie system as well as specific eponymous clans spread independently in various directions, from as many unique, but not necessarily identical, centers (p. 398).

¹ The italics are mine.

I must characterize the author's procedure as highly artificial; in fact, if consistently carried out, it would lead to quite impossible situations.

In every problem of diffusion *vs.* independent development—and our case certainly presents such a problem—the first task of the ethnologist is to analyze the similarities of the cultural traits or features in question, for on his insight into the nature and degree of these similarities will depend his initial view of the two alternative historical explanations. What, then, is the similarity on which Mr. Barbeau bases his verdict of diffusion? It is not the similarity of the social unit as such, the clan or gens, nor is it the similarity of the clan concepts of two or more tribes; it is solely the similarity of names. Two or more cultural features—in this case clans—are identified on account of sameness of name and, on the basis of the identity of this one trait, the name, are regarded as historically related by means of the process of diffusion. So far reaching a conclusion based on apparently so slight a trait would lead one to expect that the names thus found identical are most unusual or peculiar, or in some other way unique, thus precluding the possibility of their multiple independent origin. Further inspection, however, reveals no such peculiarities. Names of common American animals—Bear, Deer, Turtle—are given to clans, just as the names of animals indigenous in other countries are given to the clans which occur in such countries. Many of the animals thus used as clan names in North America occur over wide geographical areas, and so it comes to be, as might be anticipated, that several of the animals appear as clan names in different tribes. The situation is as it should be; thus there is no occasion to advance special explanations, such as diffusion, to account for the phenomena, unless other reasons may be advanced for the procedure in specific cases. Thus many of the Iroquois clans, no doubt, had a common historical origin; and the same may be said about several gentes in the Southern Siouan area, and a large number of clans in the Pueblo area. In these and other similar instances clans of identical name were doubtless often the offshoots of one original clan, or the sameness of the name was due to imitation. But the theory that the common North American clan names must be assigned single origins and specific distributions by diffusion, is as phantastic as it is unnecessary.

The same reasoning applies, of course, to the phratries. It seems especially peculiar that Mr. Barbeau, having successfully demonstrated the fundamental differences between the phratries of the Iroquois and those of the Northwest coast, should, nevertheless, feel "that the various

phratric and moiety systems, appearing sporadically in many parts of North America, may have had a common remote origin or a single center of diffusion" (*American Anthropologist*, vol. 19, 1917, p. 405). In situations such as this a wider comparative outlook will often prove of value. If phratries in the form of dual divisions associated with clans or gentes were an exceptional phenomenon, restricted in its distribution, say, to North America, this would constitute a *prima facie* justification of an attempt to correlate historically the several moiety systems of that continent. But such is far from being the case. Moieties in association with clans are all but universal in Australia and very common in Melanesia; hence one is not surprised to encounter them in some of the clan (or gentile) areas of North America, and a check is put on overzealous attempts to apply the principle of diffusion.

I must confess to a sense of keen disappointment that an American student of the thoroughness and critical acumen of Mr. Barbeau should have shown in a field with which his familiarity cannot be doubted so little grasp of the fundamental methodological principles involved in problems of diffusion and independent development.

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THE CONSTITUTION OF THE FIVE NATIONS: A REPLY

STUDENTS of Iroquoian social and political organization and folklore are fortunate in having so able a source of data as Mr. J. N. B. Hewitt. In the *Anthropologist*, vol. 19, no. 3, Mr. Hewitt criticized one of my recent publications, *The Constitution of the Five Nations*, and most ably pointed out both the faults of the native authorities who supplied my information and the errors in editing. In an earlier issue of the *Anthropologist* Dr. A. A. Goldenweiser published a criticism. In justice to the subject it would have been well and saved a possible misunderstanding if both critics had read page 12 and 13 of the introduction. There I said:

The two principal manuscripts that form the basis of this work were found on the Six Nations Reservation, Ontario, Canada, in 1910.

The first manuscript was a lengthy account of the Dekanawida legend and an account of the Confederate Iroquois laws. This material has been brought together by Seth Newhouse, a Mohawk who has expended a large amount of time and given the subject a lengthy study. His account written in Indian English was submitted to Albert Cusick, a New York Tuscarora-Onondaga, for review and criticism. Mr. Cusick had long been an authority on Iroquois law

and civic rites, and had been a chief informant for Horatio Hale, William Beauchamp, and in several instances for the present writer. Mr. Cusick was employed for more than a month in correcting the Newhouse manuscript until he believed the form in which it is now presented fairly correct and at least as accurate as a free translation could be made. The second manuscript was compiled by the chiefs of the Six Nations Council and in the form here published has been reviewed and corrected by several of their own number including Chiefs John Gibson, Jacob Johnson and John William Elliott. The official copy was made by Hilton Hill, a Seneca, then employed by the Dominion Superintendent for the Six Nations. It has been reviewed and changes were suggested by Albert Cusick. . . .

In presenting these documents the original orthography has been retained. The only attempt to record Iroquois names and words phonetically is in the notes. This will account for some variations in spelling. . . .

In the light of the conditions under which the Bulletin under discussion was presented, a compilation of native documents, criticism seems gratuitous. Especially significant is Mr. Hewitt's attempt to controvert my statement of Mr. Cusick's help. One would almost suspect this to be designed to impute a falsehood, but in the light of Mr. Cusick's assistance, this imputation would seem to fall little short of maliciousness though probably not so intended.

The reference to "a free translation" should be apparent to anyone who has read the work under discussion. Suffice to say, no translation or presentation in English can gracefully and fluently express the Iroquoian idiom. Witness Mr. Hewitt's own literal translation of the "Iroquois Cosmology." It appears in clumsy, stilted English, involved and lacking in force of expression. Literal translation robs the native thought of much of its meaning and emphasis.

Our critic's reference to wampum would seem to imply that only one sort of wampum was recognized by us, though the manuscripts clearly name elderberry twigs, scouring rushes and porcupine quills. The wampum belts described as "constitution belts" may be regarded as such even though not made during the days of Dekanawida, in this sense being as truly memorials to the founding of the League as Lincoln's Gettysburg speech is Lincoln's still even though printed in today's newspaper. The belts are old and probably native made and they have been invested with the symbolism ascribed to them—memorials of the days when Dekanawida spoke.

The lack of accuracy, consistency and forethought on the part of the authors of the manuscripts is to be deplored but even though these Indian annalists wrote clumsily it did not occur to me that of my own

initiative I should alter their writings, even for the sake of presenting them as I personally desired to see them. Mr. Hewitt must learn that if ethnologists should habitually change the myths and native manuscripts that came into their hands, in order to bring about consistency, the finished production would shrink in value. The scientist takes what comes to him from the quarry, and though it is covered by corrosion and foreign matter, he presents it as found. It is his specimen upon which he does not chisel an inscription. That is written on a separate label.

An example of native inaccuracy is quoted by Mr. Hewitt in the following: "After a journey across the lake (Ontario) he came into the hunting territory of the Flint Nation." Mr. Hewitt correctly stated that the immediate landing place of Dekanawidah would be in Oneida territory. Our Indian writer simply described things rapidly and without detailed chronological sequence, yet if some other writer had penned a line such as "After a journey across the ocean Olaf Jensen came into the forests of Minnesota," we think few critics would have deliberately gone out of their way to say that the assertion implied that Minnesota was on the Atlantic coast, especially if the statement had been made to those familiar with geography.

We accept in a proper spirit the catalogue of our own blunders but we must insist that we do not believe that in presenting the Indian manuscripts, we should eliminate their "crudity and naïvete from consideration," even to satisfy those who possess other versions of these Iroquois codes and legends. Indians who were life-long residents of their respective reservations produced the documents and stood for them. The writings represent in English, so far as they were able to make them, what they thought, believed, and lived in Iroquois. They do not necessarily represent what the present writer thinks accurate in detail or satisfactory.

Mr. Hewitt has had a large influence in directing the minds of his informants and no doubt, as he himself suggests, has contributed largely to their store of ancient lore, though we must confess it seems to us that "facts" so collected seem like re-importations; in other words, like telling one's informants what to say and how to say it. For example, Mr. Hewitt tells in his criticism how he instructed Mr. Newhouse in a certain translation of Lafitau, and says that Newhouse accepted the data and incorporated it in his code, Section 93-96. Mr. Hewitt also tells how he instructed the chiefs in the translation of certain names. It is thus evident that my distinguished critic has had an enormous advantage in previously instructing for a period of years his native informants.

They have accepted his statements as correct and incorporated them in their writings as original with them. The extent of this may be realized when it is said that some of the chiefs admitted that Mr. Hewitt wrote the introduction to the chief's version.

In our translation of the "record" staff, a cut of which was published, we simply followed the translation made by Abram Charles, a chief of the Cayugas, for Mrs. H. M. Converse, at least twenty years ago. Mr. Cusick apparently was satisfied with the translation. However, we suspected that it might be an attempt to call to mind the so-called condolence ceremony and thus we placed the picture to face that text, and with considerable difficulty, but Mr. Hewitt evidently thinks it a coincidence.

We are grateful to Mr. Hewitt's criticism, for he has pointed out a store of facts that should have been made available years ago. Modestly he refrains from more extensive criticism, but we hope to have all the necessary data when he publishes his own version of "The Constitution of the League" for which he has prepared native texts in Mohawk and Onondaga. An English parallel in Mr. Hewitt's own fluent English will then be available and, of course, be above criticism, though there will be some who will suspect that the content and the "original text" have been rigidly supervised.

Apparently Mr. Hewitt agrees with Dr. Goldenweiser's earlier criticism, and yet Dr. Goldenweiser specifically states that "The Constitution of the Five Nations is a figment. . . . It does not exist . . . either written or unwritten." Strangely, however, it appears as a coincident that Mr. Hewitt's texts and translations parallel those we have published, for the *Twenty-eighth Report of the Bureau of Ethnology* mentions his collection of "texts" in the Onondaga and Mohawk dialects,

embodying the basic principles of the civil and political structures and organization of the League of the Iroquois and data relating thereto. The following captions will indicate sufficiently the subject matter of these texts: The Constitution of the League, the Powers of Thadodaho, Amendments, Powers and Rights of Chiefs, Powers and Rights of Women, Powers of the Women Chiefs, etc.

We confess that we do not quite understand Mr. Hewitt's concluding statement,

I have purposely not given out this unfavorable estimate of Mr. Parker's recent work until it had been reviewed by one whose motives Mr. Parker might not question.

The pure love of accuracy is sufficient motive, and should have prevented

any feeling of restraint in giving out "this unfavorable estimate," until some other ethnologist has taken the initiative. We trust that this inertia of Mr. Hewitt will now be overcome and that we may be prevented from getting into further sloughs of error by his speedy publication of his own version of the "Constitution of the Five Nations." We feel sure that the faults of our own attempt will but add to the luster of the greater work that is to come.

Like Kipling's hero in *The Neolithic Age*, I feel, as I survey the bulky criticism of my bulletin, as if ". . . a rival of Solutré told the tribe my style was *outré*. . . ." But I am consoled, as every ethnologist must be who finds dozens of versions of myths and "constitutions," in the last verse of the poem, and for a pleasant thought, I present it to my critics.

Here is wisdom for your use, as I learned it when the moose

And reindeer roared where Paris roars tonight

There are nine and sixty ways of constructing tribal lays,

And—every—single—one—of—them—is—right.

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CULTURE CONTACT AND MIGRATION *versus* INDEPENDENT ORIGIN: A PLEA FOR MORE LIGHT

IN his review in this journal¹ of a volume on Oceanic mythology of which I am guilty, and more especially in a more recent discussion of it published elsewhere,² Dr. Lowie has taken a stand on the question of culture contact and migration *versus* independent origin that seems to call for a few words of explanation on his part. The matter at issue is one of such general importance and interest, and Dr. Lowie's most recent statements are so puzzling, that it would seem in order for him to bear witness somewhat more fully, to the faith that is in him.

Somewhat hesitatingly in this journal, but with complete assurance in his latest review, Dr. Lowie declares that explanations of cultural similarities and differences as due in any measure to migrations (or even culture contact!) are woefully out of date—he suggests indeed, that no sane person nowadays even condescends to consider such a discarded and worn out hypothesis, which is after all but a "curious disease" which has infected ethnological thinking during the last decade. For those who will persist in such puerile explanations, it is clear that Dr.

¹ *American Anthropologist* (N. S.), vol. 19, pp. 86-88.

² *The New Republic*, vol. XIII, no. 166 (Jan. 5, 1918), pp. 288-289.

Lowie feels only pity. Now everyone knows that a reviewer must be magisterial, but I must confess to a certain amusement at finding one so loftily denouncing, when used by others, an explanation he so constantly employs with telling effect himself; and who, after gravely rebuking an unfortunate author for "contradiction and confusion," himself sins egregiously in this very fashion a few sentences further on!

During the last few years Dr. Lowie has studied with great success the social organization of the Plains tribes. With rare skill he has analyzed the complexities of the military and other societies, has compared their forms in different tribes, and in a final summary paper, admirably drawn his conclusions as to the origin, growth, and spread of these interesting institutions. In all of this work he has constantly employed these very principles of culture contact and the influence of migration, which he so loudly decries when applied by another to Oceanic problems. On almost every page of his final summary, existing conditions are explained as due to borrowing or adoption consequent on direct or indirect contact, and these contacts, relied upon to explain the facts, are frequently substantiated and vouched for by reference to known or hypothetical tribal movements. On such evidence, amply confirmed by a mass of data on the material culture, mythology, language, etc., we have come to regard the Plains area as one in which, in addition to local developments, there has been extensive mingling of cultural elements, in part at least due to migrations. Now this is precisely the case in Oceania. A vast mass of evidence exists to show that the peoples of this whole region are, to a far greater extent than in the Plains, racially complex, and that the existing cultural conditions are the result of widespread borrowings and popular movements. To brand, therefore, as absurd, an hypothesis that the two sharply contrasted types of cosmogony in Polynesia are the result, in large part, of the mingling and interaction of two successive migratory waves, is, to say the least, amazing—the more so in that the type attributed to one of these waves, shows such demonstrable relationship to the form still retained by those members of the group yet remaining in their earlier western home. In view, moreover, of the critic's constant employment of the same hypothesis in his own field, his position seems quite illogical.

Dr. Lowie's denial of the necessity or even the value, of any consideration of culture contacts or migrations in dealing with mythological problems in the Polynesian area is the more perplexing in view of his statements in the very next paragraph. In this he declares that the author has committed the unpardonable sin—he did not tabulate all

similarities and analogies between Oceanic and American mythologies, to the end that some light might be thrown on the "question whether New World culture was deeply or at all affected by contact with alien civilization"! In other words, Dr. Lowie recognizes the possible influence in America (of necessity as the result of culture contacts or migrations) of Oceanic cultures, which would thus explain certain well known and puzzling resemblances between the two areas. He thus admits in this instance the validity of the explanation which he denies in the Oceanic area where it is, on general principles, far more probable. In one breath thus the author is soundly berated for daring to suggest, that a complex mythology may owe much of its complexity to a blending of elements as a result of direct or indirect contacts brought about by migration, in an area moreover, where many such contacts and movements are proven; in the next he is just as strongly condemned for *not* discussing at length and furnishing data upon similar supposed contacts and migrations, where none have ever been shown! May we not ask in the interests of clear thinking, that Dr. Lowie decide whether he will run with the hare or cry with the hounds! To try to do both at once usually leads to disaster.

The reviewer offers, however, not only destructive but constructive criticism. Having discarded as beneath contempt the theory of a dual origin for Polynesian cosmogonic myths, he vouchsafes to reveal to us only true faith. He accepts without reservation the conclusion that the evolutionary type is, in Polynesia, more ancient than the "creationist" form—indeed questions the sanity of anyone who should think otherwise—and then accounts for the origin of the former type, by declaring it to be a "superstructure of fine-spun metaphysical abstractions" built by "a priestly caste" on "the foundation furnished by the mythology of the laity." Now if, as he states, "intra-tribal causes" are amply sufficient to account for all the phenomena, and there is "not the slightest reason" for "dragging in" any extra-Polynesian influences, then the two admittedly different types of cosmogony must be the result of a differentiation in an isolated community, of an originally single type—the one must be the surviving beliefs of the "laity," the other the airy structure reared upon this by the priests. But alas! we are told that no "sane" person can doubt but that the metaphysical evolutionary type *preceded* in point of time, the "creationist" form—*on* which we are now told it was erected and *out of* which it grew! I must confess that I cannot quite follow here Dr. Lowie's dialectic, and cannot but have a suspicion that such "pitfalls" of "contradiction and confusion" might

have been "avoided by a very simple device, which might be heeded by ethnologists generally"—that of looking where you are going!

At this point, however, I am suddenly overcome by remorse—perhaps I have done Dr. Lowie a grave injustice and have quite falsely accused him of "contradiction and confusion." Perhaps we are talking about different things. When he speaks of a "creationist" type, he may mean, not what I have spoken of as the "creative" form, but rather that old Maori conception in which for the greater gods, no ancestry at all is given; although why the term "creationist" should be used for this is not clear. If that is the case, and *that* is what he means by "creationist," then I might agree with him that there is no *necessity* for bringing in any dual element, and that the evolutionary metaphysical type *may* be in New Zealand a local outgrowth of this—as I have indeed suggested! (p. 10). But even if this *is* what he means by the "creationist" type, a moment's reflection will make it clear that the true faith still requires of its believers unquestioning acceptance of the theory that the result precedes the cause, the child is father of the parent, and that the builder should first rear his airy spires and then proceed to put the foundations under them! Further, if by "creationist" the old Maori form is meant, Dr. Lowie directly misrepresents me, for far from saying that the evolutionary type antedates this, on the page mentioned *this* "creationist" type is referred to as "a survival of the older New Zealand belief," and the evolutionary form is contrasted with it as a "later development." Just what Dr. Lowie's meaning is in this whole matter seems doubtful, and he would appear to be in a dilemma, for either he has trapped himself in a contradiction, or he has read his text carelessly and unwittingly misrepresented his author's views. Can he suggest a *tertium quid*?

Apart, however, from all unimportant and petty detail, there is here a real question of far-reaching importance. If Dr. Lowie is convinced that the two types of cosmogonic myths in Polynesia as I have defined them, owe their divergence *in no degree whatsoever* to the interaction of two or more different currents of thought, brought into Polynesia directly and indirectly as a result of migrations, but have developed in isolation out of a single original type through the working only of "speculative fancy," I am sure there are others beside myself who would welcome a somewhat more specific and detailed exposition of his theory. This must, of course, if it is to supplant the other hypothesis, explain not only the peculiar distribution of these two special features within the Polynesian and Oceanic areas, but must also account, at least as well as the other theory, for the observed characteristics and

distribution of the other types of myths. For, if the varied forms of cosmogonic tales are purely of local "intra-tribal" growth, why should there be any need to invoke other than "intra-tribal causes" to account for differences observed in other elements of the mythology? Or, for that matter, in *any* aspect of Polynesian culture? The apparent consequences of Dr. Lowie's theory are thus seen to be really momentous, for it necessitates the assumption that the Polynesian area has been for a long period that rarest of anthropological regions—a veritable *mare clausum*; an isolated area closed to all outside influences and developing its culture in complete seclusion. It must also be noted, that explicitly for New Zealand, and implicitly for the whole of Polynesia, Dr. Lowie further assumes a "homogeneous population"; indeed he could hardly do otherwise in view of his denial of all migrations. If his postulate is true, it has, of course, a very important bearing upon the whole question. Inasmuch, however, as this assumption of homogeneity directly contradicts currently accepted opinions, together with the mass of accessible facts, Dr. Lowie must have at his disposal new and very important data to warrant him in his conclusions, and it is certainly incumbent upon him to produce these for the benefit of other students.

R. B. DIXON

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ANTHROPOLOGICAL NOTES

MR. FREDERICK WEBB HODGE, who served as editor of the *American Anthropologist* from its foundation until 1915, except for a brief interval, has resigned from the Bureau of American Ethnology, Washington, D. C. Mr. Hodge became connected with the Bureau in 1889 and since 1910 has directed its work as Ethnologist-in-Charge. Among the numerous tasks of importance may be mentioned the supervision and editing of Bulletin 30, "The Handbook of American Indians." Mr. Hodge has accepted a position with The Museum of the American Indian, Heye Foundation, of New York, which will permit him to engage personally in archaeological field work in the Southwest and elsewhere. This opportunity promises well for our science as well as for Mr. Hodge who began his anthropological work in the Southwest as a member of the Hemingway Expedition.

DR. JESSE WALTER FEWKES has been appointed Chief of the Bureau of American Ethnology, relieving Mr. Hodge, whose resignation took effect February 28.

Dr. Fewkes' archaeological and ethnological activities are well known. He was the first to develop a continued interest in the Hopi myths and ceremonies. As a natural result of this interest he has been engaged in archaeological excavation and exploration which has taken him well over the Southwest area. His work has centered however in the vicinity of the Hopi and in the Little Colorado. He has been instrumental in the restoration and preservation of several of the most important of the community house ruins.

This position of responsibility and honor is a fitting reward for the one surviving founder of modern anthropological research in America.

IN honor of the appointment of Dr. J. W. Fewkes as Chief of the Bureau of American Ethnology, a complimentary luncheon was tendered him and Mrs. Fewkes at the Smithsonian Institution on Friday, March first. Every member of the staff and all the employees of the Bureau were present. At the close of the luncheon Dr. Fewkes made a brief address, recalling the high traditions of the Bureau and outlining plans for its further development. The primary objects of its ethnologic research were defined by Dr. Fewkes as "Man in America,—where did he come

from, how long has he been here, and what has he been doing since he came?" Short speeches were made by members of the Bureau, the first speaker being Mr. James Mooney, who noted that the study of ethnology tends to bind the whole human race together by securing a better understanding of mankind.

MISS FRANCES DENSMORE, of the Bureau of American Ethnology, returned to Washington in November from field work in Minnesota.

THE establishment of a new anthropological journal has been announced. This journal is to be known as the *American Journal of Physical Anthropology*. Its founder is Dr. Aleš Hrdlička who is also the editor-in-chief. The appearance of this new *Journal of Physical Anthropology* promises to be an important event in the history of anthropology in America. There was a time when the *American Anthropologist* as the organ of the American Anthropological Association could give adequate space for the discussion of all phases of anthropological research, but the development of anthropology in America has reached a stage where no single journal can meet the needs of all investigators. The movement for differentiation began with the establishment of the *International Journal of American Linguistics* in 1917 and may be said to be consummated by the establishment of the present *Journal of Physical Anthropology*. The *American Anthropologist* is still the general organ of anthropology as a whole and will give space to general theoretical and coordinating discussions. Also, it will continue for the present, at least, to be the medium for the presentation of cultural problems, both historic and prehistoric.

The new *Journal of Physical Anthropology* will give particular attention to the anthropological problems contingent upon the war and will, for the time being, be active in supporting the work of the Committee on Anthropology of the National Research Council. The editor has associated with himself a highly representative board of editors, as follows:

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Dr. Aleš Hrdlička, who is the leading physical anthropologist in the United States, is to be congratulated upon the great service he has rendered to American anthropology and we are sure that he will receive the earnest support of all members of the Association.

DR. HERMAN K. HAEBERLIN, Assistant in the Department of Anthropology of Columbia University and the American Museum of Natural History, died at Cambridge, Mass., February 12, 1918. A later issue of the *Anthropologist* will contain an appreciation of Dr. Haeberlin's all too brief anthropological activities.

At the meetings of the Anthropological Society of Philadelphia Dr. A. A. Goldenweiser in January presented a paper on "The Problem of Culture Diffusion" and in February Dr. Boas spoke on "Thought as Determined by Social Classes."

PROFESSOR GEORGE GRANT MACCURDY, of Yale University, has been made a member of the Committee on Anthropology of the National Research Council.

YALE UNIVERSITY has been left the sum of \$50,000 under the will of Evelyn MacCurdy Salisbury, widow of the late Professor Edward E. Salisbury, to found a professorship of anthropology. The chair, which by the conditions of the will is to be occupied by George Grant MacCurdy, is to be known as the Charles J. MacCurdy Professorship of Anthropology. Additional provision is made for the maintenance of the Department and a further sum of \$60,000 is to be available in the future at the discretion of Professor MacCurdy.

MR. HOMER E. SARGENT has recently presented to the Field Museum of Natural History, Chicago, his remarkable collection of old Southwest textiles, chiefly Hopi and Navajo blankets, many of them bayetas. This

gift represents the result of judicious and discriminative collecting extended over a period of more than ten years. and includes a great deal of material that at the present time would be no longer obtainable.

DR. PAUL RADIN, Professor of Anthropology at Mills College, delivered a series of five lectures at the University of California on "The Relations of History and Ethnology." Dr. Radin's courses at Mills College have been devoted to general anthropology, North American ethnography and primitive literature.

MR. JACOB T. BOWNE, librarian of the Young Men's Christian Association College, has presented his anthropological collection to the Springfield Museum of Natural History. The collection consists of some thousands of objects collected in the Connecticut valley near Springfield. In addition to the specimens, the gift includes several hundred books on anthropology, together with archaeological cabinets, manuscripts, maps and diagrams.

DR. V. GIUFFRIDA-RUGGERI, Professor of anthropology in the University of Naples, was made on November 29, 1917, corresponding member of the *École d'Anthropologie*. On December 11, he was elected an honorary member of the Royal Anthropological Institute of Great Britain and Ireland.

IN October of last year the Hon. George Washington Grayson, of Eufaula, Okla., well known to students of the ethnology of the five civilized tribes for his interest in everything concerning the past history and present well-being of his people, was appointed by President Wilson to succeed Moty Tiger as Chief of the Creek Nation.

THOSE having copies of volume XVII, nos. 1 and 2, and volume XVI, no. 3, in good condition, may receive \$1.00 per copy for them by addressing the Secretary or Editor.

NEIL M. JUDD, Treasurer of the Association, joined the Signal Reserve Corps as a volunteer, early in the year. Dr. William C. Farabee of the University Museum, Philadelphia, Acting Secretary of the Association, has taken over temporarily the duties of Treasurer.

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KINSHIP TERMS AND THE FAMILY BAND AMONG THE NORTHEASTERN ALGONKIAN¹

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THE necessary words of caution have been duly sounded by Kroeber, Lowie, and Sapir against overconfidence in the use of kinship terminologies for the interpretation of sociological phenomena. Accordingly, the following brief report, hardly more than a sketch, on kinship in the northeast is presented not with the design of trying to reconstruct the form of an earlier type of society but simply to test an institution by determining whether the present kinship nomenclature is consistent with social conditions as we know them. It seems, consequently, strictly legitimate, without engaging in the more serious considerations of origin, to apply a system of kinship terms to social practice to learn if the ties of affinity expressed in the terms are in correspondence with the ties of actual association. If there is a lack of adjustment between the two we may, I think, assume that there have been changes in one or the other which challenge explanation. On the other hand, if the kinship system does not emphatically indicate a social structure more exogamic, or a line of descent more determined than that which prevails at the time when the present social régime is in sway, I think we may say that the correspondence in the kinship and social systems shows them to be normal and characteristic. Some initial studies of kinship are indeed greatly needed from the northeastern

¹ Read before the American Anthropological Association, New York, 1916.

✓ Algonkian region where native culture is relatively simple and ✓ social organization as loose as in the northern Woodland and Plateau areas. A complete analysis and tabulation of the terms from all of the local tribal groups is much to be desired, but such an analysis I have not been able as yet to complete. I may, however, present briefly some material bearing on this topic collected from the Montagnais, Abenaki, Micmac, Malecite, and Penobscot, all ✓ of which are organized loosely on the simple basis of the territorial family.

In the social groupings which we find prevalent among all the Algonkians of the north and east, where the family surname and the inheritance of family territories descends through the father, we ✓ should expect to find the closer lineal affinities occurring on the father's side. Assuming, also, that in the case of the death of the father of the family one of his brothers assumes authority over the children until the sons are of age, and through the practice of the levirate, the paternal uncle becomes the stepfather of the orphans, the group classification of terms would tend to be on ✓ the father's side, as it would in other cases, for instance, where patrilineal (gentile) descent is in vogue.

✓ The characteristics of a kinship system which is partially, but ✓ not radically, classificatory, appear here widely extended among non-exogamic tribes. Quite as might be expected, the Ojibwa are the only people of the culture area having a definite gentile organization, and do have a more radically classificatory system. This is quite consistent with our view as long as it restricts itself to the tribes of the northern region and, furthermore, it coincides with the rule formulated by several writers that there is considerable correlation between exogamy and the merging of lineal and collateral relatives.

Let us examine the categories among the northern tribes. In the terminology of the Ojibwa and Montagnais, the terms denoting paternal and maternal aunt and uncle, and their children, are not differentiated, nor are those denoting fraternal and sororal nieces and nephews; the father's brother is distinguished from the father, the mother's sister from the mother, and children from nephews

and nieces. Furthermore, cousins of all classes are addressed as brothers and sisters or by terms derived etymologically from these. Among the Wabanaki south of the St. Lawrence, from whom we have detailed Malecite and Penobscot lists, however, the systems are even less classificatory than the preceding and differ in the following features from those of the north St. Lawrence group. Here, paternal and maternal aunts are distinguished by different terms which are not synonymous with either father or mother, and nieces are distinguished from daughters only by the diminutive, while besides, in Malecite in the term for nephew there is some irregularity. In other respects, however, the terms are, so far as we are now concerned, similar in one aspect of their so-called non-exogamic character to those of the north St. Lawrence area.

In the foregoing remarks we have seen that in the northern Algonkian region the non-exogamous tribes have a consistently non-exogamous system of kinship terms. The same correlation has been reported as existing among the Eskimo, the tribes of the Mackenzie river, the Plateau area and to a certain extent those of California. In this connection the normality of the family territorial band as a social unit viewed in the light of kinship terms is further shown by investigation of the step-relationship terms. Investigation in social practice shows that the step-relationship, particularly the uncle-nephew one, is very close among hunting tribes, possibly because of the institution of the levirate as well as on account of the practice of sharing the inherited family hunting territories among brothers. The positive evidence would, of course, hardly be sufficient to validate the reconstruction of the family territorial group as the original social unit among all the northern tribes nor to prove its absolute fundamentality in the regions where its prevalence is characteristic. Yet it would be difficult to imagine what type of organization could have preceded the one we are considering.¹

¹ Dr. Michelson in a recent paper on Algonkian terminology ("Terms of Relationship and Social Organization," *Proceedings of the National Academy of Sciences*, vol. 2, 1916) weighs certain phenomena, the prevalence of male descent, linguistic similarity and the application of uncle-nephew, father-son terms. The material presented here agrees in part with his views.

In the case of the terms among all the northern Algonkian so far reported on, a certain degree of classification has attracted attention. It is obvious, however, from the absence of unilateral groupings in the terms that the exogamic rule is not exclusively implied here. The nature of the classifying principle is shown in the fairly close similarity prevailing among the Ojibwa, Montagnais, Penobscot, and Malecite in the terms denoting paternal uncle or aunt and grandparent.¹ Penobscot, furthermore, for example, expresses brother in general and paternal uncle by etymologically related terms. Brothers and sisters are classed with cousins on both sides. Terms for the nieces, which are cognate among the dialects of the northeast, are all similar in derivation to the term for daughter as it appears in Penobscot and Malecite. Besides these instances there are others indicating classification broadly among relatives by marriage.

Yet, considering more the character than the quantitative force of this evidence, I think it is fairly clear that the supposition of regular gentile foundation underlying the classifying tendency here is not absolutely necessary. It seems to my mind that the family territorial band, with its characteristic paternal inheritance and paternal name identity, obtrudes itself rationally as affording an idea of the under-structure of the kinship scheme in the whole area, even recognizing the variation in the degree of classification occurring in the different tribes concerned. The agreement in these systems coincides with the agreeing elements in their social structure. Hence, I see little reason for concluding that where one tribe, like the Ojibwa, differs from the others in having a gentile system intercrossed with the family grouping, that the gentile system alone should be sought for as the social factor.

The most interest, however, attaches itself to certain terms which

¹ In Montagnais, nieces on both sides and aunts on both sides are denoted by related terms. Grandmother and paternal uncle or aunt are also expressed by terms etymologically related in Montagnais; the same correlation appears in the cognate Malecite terms between grandmother and paternal aunt and mother's brother's wife, while Timagami Ojibwa denotes this irregular connection by a similarity in terms for grandfather, paternal uncle and stepfather. The other terms discussed in the section above may be found in the tabulated lists at the end of this paper.

I will soon briefly present. These seem to describe, as far as kinship terms are apt to, the conditions prevailing in practice in the family hunting bands. In view of the associations which would theoretically prevail between a man, his father, his father's brother, and his son-in-law in a family group where these men are hunting companions, the following terms carry fairly obvious indications. It is hardly necessary to say, at this point, that the associations just mentioned actually do prevail throughout these tribes, even in special cases where the levirate does not come into operation, the father's brother is consciously looked upon as something of a protector, especially so in case of a father's death or disablement. Nephews and nieces are commonly adopted by the paternal uncle and often, moreover, a young man will actually receive his hunting training from his father's brother instead of from his father.

Practical circumstances also tend to break up the natural groupings of children, nephews and nieces and weld them into new family units. An exceedingly high mortality among children as well as among adults among these miserable half-starved, half-frozen hunters,¹ causes the distribution of orphans or half-orphans not only in the interests of the children themselves but in the interest of their foster-parents as well. One of the very common principles of adoption is for the grandmother to become guardian of bereaved grandchildren. Among the Penobscot today there are four families living under this arrangement and the number of tales in which the hero is an orphan or an abandoned child adopted and raised by the grandmother is very striking everywhere in the north. I dare say that one fourth of the more important or human narrative tales among the Wabanaki and Montagnais bear this out. Accordingly, the tendency to classify such terms as grandmother and paternal aunt—both of which are potential stepmothers—by related terms, is in remarkable accordance with the practice of adoption.²

¹ The reader may judge of this matter for himself from the following instance. In 1912 I was told by the chief of the Montagnais and Naskapi at the mouth of the Moisie river, southern Labrador, numbering about four hundred, that during the preceding winter the band had lost fourteen able-bodied men and twenty-four children through starvation, freezing and disease while in the interior on their hunting grounds.

² The Penobscot informant says "Grandmother takes the place of mother."

Let us glance again at the terms. The Penobscot express the brother, the male cousins on both sides, and the paternal uncle in a similar category, the term for paternal uncle being evidently a derivation from the others. The Malecite denotes the nephews, fraternal or sororal, by a term which, I can only say with caution, seems to contain a diminutive of the stem "work," as though the relationship involves the idea of cooperative labor. Again perhaps there is something in the fact that Timagami Ojibwa, where the terms show a strong biological grouping, denotes daughter-in-law as "pleasing woman" and son-in-law as "pleasing man." Further analysis of the kinship terms would yield very surprising results, and I dare say that the next step in kinship investigation is an analytic study of the terms. So, all papers like this, which attempt to interpret or to test social conditions before exhaustive term analysis has been made, will have to be regarded only as beginnings.

The Ojibwa and Cree have been cited as offering a contradictory aspect in the matter of exogamy and kinship nomenclature, for the reason that both have the exogamic system of terms while the Cree have not the exogamic social grouping. The great majority of Ojibwa terms are cognate with those of the Cree-Montagnais-Wabanaki group. But since the latter do not have the gentile organization, nor do they observe exogamy, we may assume that the classifying tendency either has not developed among them or has not reached them. Both the Ojibwa and their congeners as well, however, do have the paternal territorial band divisions. Hence in the case of the Ojibwa, we may tentatively account for the outside influences producing exogamy and classification in certain of the kinship terms; influences accountable through dif-

LeClercq, writing as early as 1691, makes the following interesting statements regarding the distribution of orphans among the Micmac: "If there is any widow who is unable to support her children the old men take charge of them and distribute and give them to the best hunters, with whom they live neither more nor less than as if they were the actual children of the wigwam." Cf. C. LeClercq, *New Relation of Gaspesia*, edition of the Champlain Society, Toronto, 1910, by W. F. Ganong, p. 117. On p. 238 the substance of the above is given again and the author adds: "If when the father of a family is dead the widow contracts a second marriage it is necessary that the eldest son take the care of his brothers and sisters and that he build a separate wigwam for them."

fusion from peoples south of them among whom definite unilineal grouping is a characteristic.¹

The levirate, as Sapir indicates,² I also believe, is an institution deserving more attention among investigators. In this region the levirate is strictly in accordance with the requirements of the family band grouping and since the tribes of the area generally follow both institutions, it lays a great responsibility upon the levirate as a contributing explanatory factor in kinship nomenclature.³

Let us see how the levirate, applied as a test, works out in explaining the terminology of the Timagami Ojibwa who not only are organized on the basis of the family territorial band, but who also have the exogamic gentile system.

¹ In another paper, "Family Hunting Territories and Social Life of Various Algonkin Bands of the Ottawa Valley," *Memoir 70 (Anthropological Series No. 8) Geological Survey of Canada*, I have given some results of a study of a small band of Ojibwa at Lake Timagami, a northern branch of the tribe which has extended its habitat in recent times into northern Ontario. Here the terms abound in classification, but the gentile organization has weakened. I have indicated it as my opinion in this paper that we can trace the weakening of the gentile organization in the historical changes undergone by the band, through dissociation with the older gentile stock. The facts are fairly consistent here, for the general loss of complexity in economic as well as social and religious life shows how the Timagami people have conformed, through intermixture with non-gentile Algonkian, to a new environment. Moreover, all of the potential relationships of the family territorial band are present in the terms here in about the same way as in the related tribes to the north and east who have the same local grouping but no exogamy or gentile descent.

² E. Sapir, "Terms of Relationship and the Levirate," *American Anthropologist* (N.S.), vol. 18, no. 3, 1916.

³ Although the institution has not been systematically reported on among all the tribal groups of the northeast we have nevertheless a number of authentic statements. The writer found that the Montagnais of the lower St. Lawrence used to require a widower to marry his deceased wife's sister "as his next choice, so that his children would not be liable to maltreatment." LeClercq (*op. cit.*, p. 238) says of the Micmac: "After the death of one's brother it is permissible to marry his wife in order that she may have children of the same blood if she has not had any by her first husband." LeClercq (*Cf. First Establishment of the Faith in New France*, translated by J. G. Shea, N. Y., 1881, vol. 2, p. 134) elsewhere refers to the custom among the northern Indians and says that they often take several sisters that they may agree better.

Among the Penobscot and modern Micmac the custom is well known through tradition.

stepson, <i>ningwə'skawan</i> ¹	stepdaughter, <i>ninda'niskawan</i>
son, <i>ningwi's</i>	daughter, <i>ninda'nis</i>
brother's son, <i>nindo'ziməs</i> ²	brother's daughter, <i>nindo'ziməs</i>
sister's son, <i>nindo'ziməs</i>	sister's daughter, <i>nindo'ziməs</i>
stepfather, <i>nici'cēc</i>	stepmother, <i>ninu'cēc</i>
paternal uncle, <i>nici'cēc</i>	maternal aunt, <i>ninu'cēc</i>
father, <i>nu'se</i>	mother, <i>ninga'</i>
father's brother's child ³ <i>nidjki'weka-</i>	brother, <i>nidjki'we</i>
<i>wan</i> (masculine)	
mother's sister's child, <i>ninda'wema-</i>	sister, <i>nindawe'ma</i>
<i>kawan</i> (feminine)	

A feature not distinguished here, the presence of which would settle absolutely the question of the influence of the levirate in the groupings of these terms is differentiation of man's brother's child and man's sister's child, since the latter could not come into the filial relationship by the custom of the levirate though he could by being adopted by his maternal uncle. This is the case both in social practice and in the kinship indications among the Wabanaki tribes, where the differentiation of the terms involved confirms the levirate. I think, however, that possibly the inclusiveness of this category is due to the frequency with which wholesale adoption of a man's sister's and especially of a man's brother's children takes place, on account of frequent orphanage. The above terms in no uncertain sense express the potential relationships of the levirate among the Lake Timagami Ojibwa. A similar deduction may be drawn from the terms of the Montagnais while the complete collateral differentiation in the terms involved in Penobscot and Malecite renders practically certain the deduction of the levirate.

The kinship terms involved in the levirate and nepotic relationship in Penobscot are as follows:

¹ The Ojibwa and Montagnais suffix *-kawan* means "not of pure (blood) descent."

² Literally "my (dear) little child," contraction of *nin-d-awa's-imis*; *awa's-i's* "child." Cf. also Cree *ntojim* nephew (p. 200), *n't'awāssimis* "my child" (p. 128), Father A. Lacombe, *Dictionnaire de la Langue des Cris*, Montreal, 1874.

³ These are the general terms. Through an oversight this term was omitted from the list given in the paper on the Timagami band referred to on the preceding page.

son, <i>ne'mon</i>	daughter, <i>ndu's</i>
brother's son, <i>ne'ma'ni'mi's</i> , "my little pet son"	brother's daughter, <i>ndu'zi'mi's</i> , "my little pet daughter"
sister's son, <i>nda'wazəm</i> , "my child" ¹	sister's daughter, <i>səmə's</i>
stepson, } <i>nda'pa'k'wəni'gan</i>	"the one I wrap about and protect"
stepdaughter, }	
father, <i>nəmi'təŋwəs</i> , "my generator"	mother, <i>ni'ga'wəs</i> , "one who bore me"
father's brother, <i>ni'djaluk</i>	mother's sister, <i>ngi'zi's</i>
stepfather, }	stepmother, }
grandfather, }	grandmother, }
brother	sister, }
half-brother, }	half-sister, }
cousin (man speaking), }	cousin (man speaking), }
paternal cousin (male), }	paternal cousin (female), }
maternal cousin (male), }	maternal cousin (female), }

Through this series of terms one can perceive the underlying significance of the levirate in specific cases. Thus in the case of my father's death, my mother could marry my father's brother or else her sister's husband, both of whom are *ni'djaluk* (Penobscot) to me, in other words potential stepfathers through the operation of the levirate. So also the man who takes his deceased brother's wife and her children (his nephews and nieces) calls them *ne'ma'ni'mis* and *ndu'zi'mi's* terms derived from those for son and daughter. Or he may term them descriptively stepson and stepdaughter. Similarly among brothers and sisters, half-brothers and sisters and cousins the terms are in harmony. The other terms also seem to define their relationship in the light of the levirate.

In a region like that which we are investigating, where exogamy, it appears, can hardly be thought of as a sole factor in the development of relationship terms, we are left to the alternative of regarding the institution of the family territorial band and some such feature within it as the levirate, as influencing agents. As Dr. Sapir intimates in general, in an attempt at explanation it seems about as plausible, in the case of this particular group of tribes, to lay stress upon the influence of the levirate itself in its local economic setting, as on any other single social principle.

¹ This denotes the period just out of babyhood.

² -s *kwe* feminine suffix.

Dr. Lowie¹ in a recent article has interpreted the Cree and Ojibwa phenomena as due probably to the loss of an earlier exogamic grouping among the Cree. It seems to me that a more plausible suggestion is that the Cree and Ojibwa and the other northern and eastern Algonkian, whose organization possesses the family territorial band, were none of them originally exogamic, but that the paternal tendencies of the family band universal among them are connected with the form in which the kinship terms appear, a somewhat classificatory form throughout the whole area.

Ojibwa, then, is classificatory in kinship, not because the tribe is exogamic, but for other reasons. In the nature of a suggestion without further details for a basis,¹ I may say as before that the levirate and the common northern type of society appear to fill the explanatory requirements.

I have just given my main conclusions. Since, however, I have undertaken in this paper to present a little material from a somewhat overlooked field, the following points may prove suggestive. In talking over kinship terms and their uses with informants I have been impressed with the probability that the agencies which operate toward the adoption of some particular category in nomenclature are often trivial ones; much more trivial than some investigators might care to admit. In the area under discussion I think it has become evident as a possibility that local linguistic usage, resulting in differences within an area, has developed from causes which were local and relatively trivial. While linguistic analysis of terms is in this regard an absolute necessity, does it not seem that the variable use of diminutives, of the classifying terms, even of the vocatives, may have arisen through local usage in terms of baby-talk, endearment and respect? In the northeast where the bonds of kinship are loosely recognized in the family group, as well as among friends, the use of terms is correspondingly loose, and so we may account for some of the irregularities which occur in tribal systems over even a restricted area.

It is interesting to note a few more points of character in the

¹ "Exogamy and the Classificatory Systems of Relationship," *American Anthropologist* (N.S.), vol. 17, no. 2 (1915), p. 235.

kinship systems of these tribes. Vocatives in general in all these dialects occur with greater frequency in the terms involving address within the immediate family as shortened forms of the non-vocatives denoting endearment. The final syllable is changed to *i*. A trace of the source of some of the vocative forms is perhaps betrayed by the baby-talk terms which appear here and there in the lists. It is true of the Timagami Ojibwa that several of the vocatives do come from such affectations, for example, *djudju*, vocative for "mother" (from *noni*, "to suckle," or *totos*, "breast") and *data*, vocative for "father." The former we see again as *numu'n*, a baby-talk vocative in Penobscot and a formal term in Abenaki (list B) and the latter as *dada(n, t)* in similar circumstances. The mere fact that these terms appear in three functional series, as simple baby-talk addresses, as formal kinship terms, and as vocatives in a close linguistic group, is by itself suggestive in this respect.

With some exceptions, to be noted, reciprocal terms are almost completely wanting in the Eastern Woodland area, as has already been noted by Lowie.¹ They are only sparingly employed among the various kinds of brothers-in-law and sisters-in-law, and between husband and wife, in other words between those related by marriage. Another negative point of importance is that here no radical change in the terms takes place to denote deceased relatives. The age differentiation in terms for brother, sister (and even for cousins on both sides in Penobscot and Malecite) include three forms; general terms for brother and sister, a distinct one for elder brother and elder sister, and a common term for younger brother and younger sister. In the first and second categories there are separate forms according to the sex of the speaker.

The terms themselves seem to fall into two categories. In one of these we have generic stems like Penobscot *no''kəm*, *nidji'e*, *ndus*, *nada'ŋk'w*, *nsəgwu's*, expressing fundamental relationships and from some of which certain other specific terms are derived. They include quite a number of the classificatory terms in the list and these show more uniformity throughout the stock than do the

¹ R. H. Lowie, "Historical and Sociological Interpretation of Kinship Terminologies," *Holmes Anniversary Volume* (1916), p. 296.

others.¹ In the other category are composite terms which furnish for the most part the points of differentiation which we notice in designations among the dialects of the region. In the latter group of words analysis is often possible. On the whole it seems that here, where the etymological possibilities are retained, we have an illustration of terms of more recent origin.

In the following list collections of terms denoting kinship are presented from the principal northeastern tribes. Before offering lists like these for objective use, however, a word or two of explanation concerning the sources is desirable because it often makes some difference when, where, and under what circumstances the vocabularies are taken down. Individuals frequently differ in term usage; synonyms are optionally given or passed by, while confusion occasionally reigns in the mind of him who is trying hard to give definite renderings to terms which are not in common use. Informants sometimes even surprised me by contradicting each other in regard to the limitations of brother-in-law and sister-in-law terms and those for nephew and niece. In order not to overlook specific terms, especially those for the more remote degrees of relationship, I employed several native authorities in the preparation of the lists which I obtained myself. In these also the term analyses and the translations are the combination of chosen interpreters' knowledge and grammatical analysis.

The terms in the first column are Penobscot. They were recorded at Oldtown, Maine, with the help of Newell Lyon. List A, St. Francis Abenaki, has been arranged from the vocabularies given by Sosap Lolô (Joseph Laurent) in his book on the Abenaki language.² The orthography has been altered from the original, in accordance with my own notes in this dialect, to correspond to the authorized system in use among students of American linguistics. List B was taken down from Maude Benedict at Lake George in 1908. The circumstance of interest here is that some of the terms are derived from baby talk (terms 1, 2, 5,) and that some of the

¹ Morgan, and more recently Michelson, (*op. cit.* p. 297) have noted the underlying similarity in the terms.

² Joseph Laurent, *New Familiar Abenakis and English Dialogues*, Quebec, 1884.

regular formal terms are replaced by diminutives of the baby-talk terms (3, 4, 7, 11, 12). The question presents itself, is this usage a primitive characteristic or is it a reminder of linguistic decay? The occurrence of cognate terms in formal use was a feature of Mohegan in southern New England,¹ and it may be true that idiomatic features like these which were general to the southern New England dialects were conveyed to the composite Abenaki people in one of the migrations which carried the central New England tribes, like the Pigwaket and Pennacook, to the St. Francis village almost two centuries ago. On the other hand we should not overlook the fact that the process of culture decline has gone further among the Abenaki than among the others of the northeastern group. The Malecite list was recently obtained through the combined efforts of Gabe Paul, Mrs. Nellie Tomer, and Gabe Perley. Little requires at present to be said about these terms except to note that they appeal rather more closely to those of the Micmac than do the other Wabanaki terms. The terms in the Micmac column are the contribution of Mrs. Frank Lewis, who is a native of the village of Restigouche, New Brunswick. Morgan's list of Micmac and Malecite terms² differs from these in the identity of several of the niece and nephew terms. The only other differences are due to his system of spelling. The Montagnais list was taken at Escoumains, Quebec, in 1915 from Mrs. Joseph Nicolar.

¹ See footnote 4, page 156.

² L. H. Morgan, "Systems of Consanguinity and Affinity," *Smithsonian Contributions to Knowledge*, vol. xvii. (1871), pp. 293 *seq.*

	Penobscot	St. Francis Abenaki		Malecite and Passamaquoddy	Micmac	Montagnais (Escoumains Band)
		List A	List B			
father.....	nəmi'iq'wəwəs ¹	nmi'təgwə's	ndada'n ²	nəmi'tə'k'w's (ndada'd)	nulc	nola'wi' ¹
mother.....	nī'gə'wəs ³	nīgawə's	nunū'n ⁴	nī'gwəw's	nki'tc	nəqə'wi' ³
stepfather.....	nəmo'su'məs	n'ɔ'dji'k' ^w	ndadani's ⁵	nəmo'su'məs	nī'tckami'tc	
stepmother.....	nō'kəmə's ⁶	nok'amə's	nunūni's	nō'kəmə's	nō'ɣəmi'tc	
grandfather.....	nəms'su'məs	nmadho'm ⁷	nəmo'm	nəmo'su'məs	nī'tckami'tc	nəmoču'm
grandmother.....	nō'kəmə's	nokəmə's	nokəmə's	nō'kəmə's	nō'ɣəmi'tc	nəqu'm
paternal aunt.....	nō'kəmə's	nokə'm	nunūni's	nō'kəmə's	nsugwə's	noquum'c
maternal aunt.....	ngi'zi's	nokəmə's	nukū'm	ngi'zi's	nələ's	nus
mother's brother's wife.....	nō'kəmə's	nokə'm	nukū'm	nō'kəmə's		(descriptive terms)
father's brother's wife.....	ngi'zi's	nokəmə's		ngi'zi's		
paternal uncle.....	nī'djə'luk	n'odji'k' ^w		nī'djə'luk ^w	nkla'muksi's ⁸	noquum'c
maternal uncle.....	nəza'si's	nza'si's	nza'si's	ngla'muksi's	nkla'muksi's	nus
mother's sister's husband..	nī'dzə'luk	n'ɔ'dji'k' ^w	ndadani's	nī'djə'luk ^w	nkla'muksi's	(descriptive terms)
father's sister's husband...	nəza'si's	nza'si's	ndadani's	ngla'muksi's	nkla'muksi's	

¹ Literally "my generator."

² In this case the term most commonly used is derived from baby-talk. The formal term, however, is well understood. The Penobscot also know these as baby dialect.

³ Literally "the one who bore me."

⁴ Literally "my breast," again a derivation of baby-talk. Cf. also Mohegan nə'nə'ŋ "my mother," "Notes on the Mohegan and Niantic Indians," F. G. Speck, *Anthropological Papers of the American Museum of Natural History*, vol. III, p. 193 (1909). Morgan also gives a Mohegan term for grandfather which falls in this class.

⁵ Literally "my little dada'n," -is is the diminutive.

⁶⁻⁸ Ordinarily denotes "old, antiquated."

Again a case of the use of baby-talk, an attempt to pronounce nəmo'sum.

⁸ Possibly a derivation of gla'mə "to stick something onto something," with -sis diminutive; literally "the one to whom I am fastened."

	Penobscot	Abenaki St. Francis List A and List B	Malecite and Passa- maquoddy	Micmac	Montagnais (Escoumains Band)
m. speaking	brother (general term)	<i>ni' dji' a'</i> ¹ { <i>ni' dji' e</i> <i>nada'ngwus</i> ² }	<i>ni' dji' a'</i> <i>nadogwe's</i> <i>ni' dphso'</i> ⁶	<i>ni' dji' e'</i> <i>no'gammau</i> ³	<i>ni' ci' m</i> <i>ni' ci' mqa'waan</i> ⁴ <i>ni' ci' m</i>
w. speaking	brother male cousin younger brother (m. or w. speaking) . . . older brother (m. or w. speaking)	<i>nda'tnum</i> ⁵ <i>ndoka'ni' mi's</i> <i>nza'si's</i>	<i>ndapso'</i> <i>ni' lsemi's</i> <i>nabhe'zi's</i>	<i>ni' dji' e'</i> <i>ni' ci' gonam</i> <i>ni'si's</i>	<i>ni' ci' m</i> <i>ni' ci' m</i> <i>nasi'e</i> (<i>nastacqe'waan</i> older cousin) <i>ni' ci' m</i>
m. speaking	sister (general term)	<i>nabhe'natum</i> ⁷ <i>nada'ng'wes'kwes</i> ⁸ { <i>ni' lse'kes'u</i> <i>nada'ng'wsi's</i> }	<i>nabhe'natum</i> <i>ni' lse'kes</i> { <i>ni' lse'kes</i> <i>ni' da'k'wsi's</i> }	<i>ni' dji' e'</i> <i>ni' ci' gonam</i> <i>ni' dji' e'</i> <i>ni' ci' m</i> <i>ni' ci' m</i> <i>nemi's</i>	<i>ni' ci' m</i> <i>ni' ci' mqa'waan</i> <i>ni' ci' m</i> <i>ni' ci' m</i> <i>nami'e</i> (<i>nami'eqa'waan</i> older female cousin)
w. speaking	sister female cousin sister female cousin younger sister (m. or w. speaking) . . . older sister (m. or w. speaking)	<i>nabhe'natum</i> ⁷ <i>nada'ng'wes'kwes</i> ⁸ { <i>ni' lse'kes'u</i> <i>nada'ng'wsi's</i> }	<i>nabhe'natum</i> <i>ni' lse'kes</i> { <i>ni' lse'kes</i> <i>ni' da'k'wsi's</i> }	<i>ni' dji' e'</i> <i>ni' ci' gonam</i> <i>ni' dji' e'</i> <i>ni' ci' m</i> <i>ni' ci' m</i> <i>nemi's</i>	<i>ni' ci' m</i> <i>ni' ci' mqa'waan</i> <i>ni' ci' m</i> <i>ni' ci' m</i> <i>nami'e</i> (<i>nami'eqa'waan</i> older female cousin)

¹ Derived from *i' dji'* "to go in company with."
² Derivative of *nada'ng'w* (see term for sister's husband).
³ Derived from *akame'* "across," in the relative sense "collateral."
⁴ *-qa'wan* "not by blood descent."
⁵ Literally "my close relative."
⁶ Literally "my man (reflexive)."
⁷ Literally "my woman."
⁸ Literally "my little woman."
⁹ *-s'kwes*, and *-skwa*, feminine suffixes.

	Penobscot	St. Francis Abenaki		Malecite and Passamaquoddy	Micmac	Montagnais (Escoumians Band)
		List A	List B			
sister's husband (m. speaking).....	nada'ŋk ^ŋ _w	nado'g ^w		nəmə'gədem ¹	nida'k ^w (nəmə'klem)	(descriptive, explanatory terms)
husband's sister.....	nada'ŋk ^ŋ _w	nado'g ^w		nada'k ^w	nida'k ^w	
wife's brother.....	nada'ŋk ^ŋ _w	nado'g ^w		nəmə'gədem	nida'k ^w (nəmə'klem)	
brother's wife (w. speaking).....	nada'ŋk ^ŋ _w	nado'g ^w				
brother's wife (m. speaking).....	ni'lamu's	ni'la'm		nada'k ^w	nida'k ^w	nik'an
husband's brother.....	ni'lamu's	ni'la'm		ni'lamus	ni'lamus	
wife's sister.....	ni'lamu's	ni'la'm		ni'lamus	ni'lamus	
sister's husband (w. speaking).....	ni'lamu's	ni'la'm		ni'lamus	ni'lamus	
wife's sister's husband.....	ni'lcu's ² (ni'dji'e)			ni'lamus	ni'lamus	
husband's brother's wife.....	ni'lamu's			ni'lcu's	ni'lcus	
wife's brother's wife.....	ni'lamu's			(nəzi'wes')		
husband's sister's husband.....	nada'ŋk ^ŋ _w			ni'tse'hes		
daughter's husband's father.....	ndo dem ³			ndo dem	ndo dem	
mother.....	ndodemi'skəw			ndodemi'sk ^w	ndodemi'sk ^w	
son's wife's father.....	ndo dem			ndodemi'sk ^w	ndodemi'sk ^w	
mother.....	ni'zwi'ek ⁴	ni'zwi'a'k	nsanəmba'm ⁵	ni'zwi'ek	ni'zwi'ek	nəbe'm ⁵
husband.....	ni'zwi'ek ⁴	ni'zwi'a'k	nəbanamul'm ⁶	ni'zwi'ek	ni'zwi'ek	nəbe'm ⁵
wife.....	nda'los'uk ^w	wəzi'ini't nsəm		ndlo's'uk ^w	ndlo'zək	lenals'm
daughter's husband.....	nsəm			nəzə'm	nlaswə'sk ^w	
son's wife.....	nzi'li'hos ⁷	ndji'lo's	ndzi'lu's	nzi'li'hos	ni'licc	nəcə'c
wife's husband's father.....						
wife's husband's mother.....	nəzəgwə's	ndzəgwə's	ndzakə's	nəzə'gwus	nlsogwi'i'dji'tc	nico'qas

¹ Derived from a profane sexual term in modern use.

² A term of considerable sociological importance since the correlatives are potential partners. The association frequently figures in the myths. Cf. S. T. Rand, *Legends of the Micmacs*, p. 357, "wechoostijik (the two men whose wives were sisters) were on the best of terms and were much together." Authors' minds often run in the same channel for we note that C. G. Leland, *Algonquin Legends of New England*, p. 355, also speaks of this friendship. He says "two men whose wives were sisters (wechoosjik) were on the best of terms and much together."

³ Literally "my comrade of another descent." The term is used modernly among the Penobscot to designate a friend of another tribe or race in the ordinary sense, for instance "my white man friend" when an Indian is speaking. In the text of one of the myths the term *udo/demago* "his own master" is used in reference to a dog and his master. The form here is cognate to the common Algonkian *n'tol, m.*

⁴ Literally "the one who lives with me."

⁵ Literally "my man."

⁶ Literally "my woman."

⁷ -(h)os denotes a mild degree of scorn (for example the proper name *Sakhos* "that old Jacques (Sak)"). Evidently something like "that old marriage connection of mine" is the meaning conveyed. Cf. *ndatzi'udamon*, "I marry into such and such a family."

	Penobscot	Abenaki St. Francis List A and List B	Malecite and Passamaquoddy	Micmac	Montagnais (Escou- mains Band)
son.....	<i>ne'mon</i>	<i>namu'n</i>	<i>nogwut's</i>	<i>nquas</i>	<i>nequo's</i>
daughter.....	<i>ndu's¹</i>	<i>ndos</i>	<i>ndos</i>	<i>ntlus</i>	<i>ndanc</i>
stepson.....	<i>nda'pa'k'wut'gan²</i>		<i>nda'pa'k'wut'gan</i>	<i>nquas</i>	<i>nequas</i>
stepdaughter.....	<i>nda'wagazam³</i>		<i>ndalo'k'ni's (mal.)</i>	<i>ntlus</i>	<i>ndanc</i>
sister's son (m. s.).....	<i>nemex'ni'mi's⁴</i>	<i>namuni'mi's</i> (<i>namuni'mi'za'm</i> "younger")	<i>ntaw'wazam</i>	<i>nalu'ks</i>	<i>nto'cam⁵</i>
			<i>namani'mi's</i>		<i>nto'cam</i>
brother's son (m. s.).....	<i>nemex'ni'mi's</i>	<i>namuni'mi's</i>	<i>ntaw'wazam</i>	<i>nalu'ks</i>	<i>nto'cam</i>
	<i>nda'wagazam</i>		<i>ndosi'mi's</i>	<i>nsam</i>	<i>ntocamsque'm⁵</i>
sister's daughter (m. s.).....	<i>sam's</i>	<i>ndosi'mi's</i>	<i>ndosi'mi's</i>		<i>ntocamsque'm</i>
	<i>ndosi'mi's⁶</i>		<i>ndalo'k'ni's</i>	<i>nsam</i>	<i>ntocamsque'm</i>
brother's daughter (m. s.).....	<i>sam's</i>	<i>ndosi'mi's</i> (<i>ndosi'mi'za'm</i> "younger")	<i>ndosi'mi's</i>		<i>ntocamsque'm</i>
wife's or husband's	<i>nemex'ni'mi's</i>				
wife's or husband's	<i>nduzi'mi's</i>				

(160)

¹ Possibly a worn-down form of *n-d-awax's*, "my creature" in the literal sense. Cf. *n-d-awaxi's*, "my child" and terms for niece and nephew above. Speakers sometimes distinguish age grades as *ndu'zi's*, "my younger daughter."

² Literally "one whom I wrap up and protect." Cf. *ndadape'gwamən* "I cover up with a wrapping of skins."

³ Literally "my pet child" (*awaxi's* "little child" (Penobscot).

⁴ *-imi's* diminutive of endearment, literally "my dear little son."

⁵ Literally "my female child."

⁶ Literally "my dear little daughter."

	Penobscot	Abenaki St. Francis List B	Malecite	Micmac	Montagnais (Viscount- mains Band)
great-grandfather }	<i>klci' nɔmo'su'mas</i>	<i>klci' nɔmoumi't'sɪ</i>	<i>bɪ'tɪɔwi'²</i>	<i>klci'</i>	<i>nɪd'usqutaba'n</i>
great-uncles }			<i>nɔmo'sumi's</i>	<i>nɪ'tɪckami'tc</i>	<i>moɔw'm</i>
great-grandmother. }	<i>klci' nɔ'kaməs</i>	<i>klci' nɔkɔmɔ's</i>	<i>bɪ'tɪɔwi'</i>	<i>klci'</i>	<i>nɪd'usqutaba'n</i>
great-aunts }	<i>nkwɛ'nɔəs</i>	<i>nɪ'sɔs</i>	<i>nɔ'kaməs</i>	<i>nɔ'ɔmil'tc</i>	<i>noqu'm</i>
grandchild (male or female) }		<i>klci' nɪ'sɔs</i>	<i>nɔgwɛ'nɔs'</i>	<i>nodjɪ'tc</i>	<i>nosɔ'm</i>
great-grandchild }					

¹ Literally "great (big) my little grandfather."

² Literally "under (adjective) my grandfather."

NOTES ON ACOMA AND LAGUNA

By ELSIE CLEWS PARSONS

THE following notes are the result of brief visits to Acoma and Laguna in January, 1917. The notes are fragmentary not only because of the shortness of my stay but because, in the case of Acoma at least, I had to contend against extreme distrust of Whites. Although my introduction was of the best (I was with Zuñi who had connections in Acoma) and although I was most hospitably entertained by the Acoma household who took us in and who kept me on after my Zuñi friends left, I was unable to overcome the distrust altogether and much of my time was squandered in merely trying to differentiate myself from the picture-taking tourist or from the Washington representative from whom every ceremonial or intimate detail of life is to be hidden. The "cacique" to whom I had taken a present of tobacco broke off in the middle of a folk tale, for example, to get my assurance that I would not send the tale to Washington. Rather than have had me witness the masked dance (parrot dance?) they had planned for the day after that set for my departure I more than suspect they would have changed their program. In fact when I said I was going to stay they told me the dance was off. No Whites are allowed at present to see the masked dances either at Acoma or Laguna, dances held, mind you, not in an *estufa* or ceremonial room but out in the plaza.

The dance I did see in Acoma was maskless and, as a Zuñi critic would say, very "unfinished." It was given on January 27 and 28 to celebrate the installation of the officers—the *tapup* or *tapopo* (governor), the two *tenientes*,¹ the three war chiefs (*tsatiahucha*) (*piscal*, "Mex."; sheriff, "Amer.") and their two *cusinero* (cooks).

¹ "Second lieutenant governor" and "third lieutenant governor." The latter is also called *aioranti*, a "Mexican" word.

All these officers are chosen annually by the "cacique" ¹ and installed on December 30 and it is customary for them subsequently "to give a dance," to celebrate. By the Mexicans the celebration is called *fiesta del re*, and by Acoma, *koachansiwatsask*. The dance that was given was the *hoinawe*. It is the dance customarily given on this occasion. On January 23, I arrived in Acoma and that night and subsequent nights in the two estufas that were presenting the dance the men assembled from about seven o'clock to midnight to practise the songs, songs composed, as at Zuñi, for the occasion. The women who were to dance, but not to sing, did not meet with the men. The Zuñi in my party was invited to attend, but neither his wife nor I were allowed to be present. The men were summoned to the dance by the officers who walked crying out the summons through the three avenues of the town, *tokehaliuchenāi*, "North Row," *tokesthunahatiuchenāi*, "Middle Row," *tokekwinihatiu*, "Last." Four circuits are supposed to be made, one officer following the other. The custom characteristic of other pueblos of calling out the orders from the house-top is not found at Acoma.

About seven a.m., January 27, the church bell began to ring and the *kahera* (church drummer) to proceed up and down the three avenues. Four times the bell has to ring and four times the drummer makes his rounds to summon the people to the church. During the interval while I was strolling over the rocks to the south of the town I overheard as I stood near the wooden cross ² at the head of the "burro trail" (*kwiana?*) the unmistakable drone of prayer. It came from the edge of the cliffs facing east. I had of course to withdraw discreetly and by whom the prayer was said or whether it had any special connection with the ceremonial of that day or was merely a daily sunrise prayer I was unable to ascertain.

¹ My Acoma host understood this term but he did not himself make use of it. *Hochení* or *huchāini* meaning person in authority was the term used by Laguna and Zuñi informants in referring to the "cacique" of Acoma.

² Formerly at this cross the people used to go out and greet kneeling the high priest (bishop?) of the Church.

To the lower arm of the cross were tied dry wisps of plants. On "Cross day" in May they tie flowers to the crosses.

On Ash Wednesday everyone is marked on the forehead with a cross of ashes.

About eight o'clock I went to the church. "Shake hands with the governor. He will be standing at the door," said my host. Standing at the door was a group of eight men, several carrying canes. With one or two of the cane bearers the men and women as they came in would shake hands. Then the women went to the left, *i. e.*, south side of the church and the men to the right or north side. About thirty women and little girls stood one behind the other close to the wall of the church. A few stood irregularly, like some of the men, nearer the door. The twenty or more men and women who kept the space near the altar filled also divided, women to left, men to right. People came and went irregularly as in any Catholic church. On entering the sign of the cross was made and a prayer was said. Some knelt as they prayed. One woman remained kneeling in prayer. (She also read from her prayer book. In dress she was entirely Americanized and she spoke English fluently. She was the sister of one of the *tenientes*.) The hat-wearing men removed their hats, but the men who wore bandas¹ did not remove them.

As I entered a prayer was being said at the candle-lit altar, by whom I did not learn. It lasted about ten minutes. Then for about five minutes, one of the *tenientes*, not the governor (the governor was distinguishable by a silver badge engraved "Governor of Acoma") standing with the others just at the threshold, prayed, this prayer being assented to every few seconds by the other officers. This prayer concluded the service and the church emptied. The officers continued to stand together in a little knot for a few minutes and then they too scattered.

It was an officer, I surmise,² who had said the altar prayer. At any rate it was not the priest. He comes to Acoma only on September 2, the day of the patron saint, St. Stephen, or when he is especially summoned. Acomita, where the bulk of the people live, except from November to February, the priest visits twice a month. To the saint's dance, I may say, you, a White, are cordially

¹ The wearing of bandas is not as general as at Zuñi and long hair done up in the queue appears infrequent. As for the women, more of them are Americanized in their clothes too than Zuñi women.

² Whether or not there is a "sextana" at Acoma as at Laguna I did not learn.

invited. After it the people return to their farms at Acomita, to return again in November to Acoma for their winter ceremonies.¹ Only a few households live permanently at Acoma.

I return to the *fiesta del re* and to an account of the *hoinawe* dance which followed the church service. About 10 a. m. the officers proceeded about town calling out the order to come to the estufas to prepare for the dance. At 12.15 appeared the group from the estufa at the west end, north side, of "North Row," estufa B, I will call it. The first to descend the ladder was the drummer. After he had drummed a couple of minutes the men of the choir descended and formed into three or four lines, eight or ten to the line. Next to descend were four women. And after them an elderly man, who carried in his arms certain feather and worsted paraphernalia. On the ground he proceeded to fasten to the back of two of the women the wand on which were feather pieces to decorate their flowing hair and to distribute to all four the feathers they were to carry upright in their hands. Meanwhile the van of the choir of men had been reënforced from the street by two lines of about twenty boys of all ages from eight to sixteen, and all had begun to sing and to move forward with the usual dance step. The women led single file by the old man, the two head-befeathered women in the lead, caught up with the choir and with the woman's shorter dance step began to pass along from one line of the choir to the other. The group proceeded singing up the street, one or another dancer whooping or shouting now and then, until they came to the dance plaza or rather floor where every Acoma dance takes place. This place, *kakati*, is at the junction of "North Row" and a cross street about one hundred and fifty feet long and one hundred feet wide leading into "Middle Row." Arrived at this place the choir formed into a group of concentric rings, the boys on the outside and the women weaving slowly in and out except in the outermost ring of boys. All continued to dance and all the males, some of the boys excepted, to sing.² After about fifteen minutes the song

¹ As a consequence of these migrations Acoma takes on, I think, a kind of sacrosanct character, and ethnological pursuit becomes particularly difficult. It would be easier, I venture to say, in Acomita.

² There were Navajo words in one of the songs I was told. *Acaye* (come here) *shi* (me) *yato* (good).

ceased and the two women without the feather ornament in the hair were led out by one of the men of the group and down the street and back to the estufa. The other two women then took a position on the north side of the choir, *i. e.*, between the choir and the house wall. Facing them was the older man who had taken a director's part in the preceding movements. The choir closed up into a more compact group as it was now merely to sing and not to dance. The drummer left his position on the outside and stood in the center. The two women faced each other and as they stepped from foot to foot in the same place they moved up and down their arms bent at the elbow and the lower arm at right angles to the body. The two right arms were up when the two left arms were down. After a minute or two of this they turned and, facing the choir, they side-stepped away from each other a few feet, holding both arms in the direction of the movement. Then they moved back into the first position. Dancing *vis-à-vis* they again side-stepped and each position was repeated five or six times. In the middle of the repetitions the arm movement in the *vis-à-vis* position was varied two or three times with a wrist movement. Each would wave her eagle feathers up and down, at one time both hands moving together and in the same direction as the hands of the *vis-à-vis*, at another time the movement of the hands alternating. The man who faced the women stood the whole time on the same spot, beating time with his feet rather than dancing, and moving his arms like the women only less so. His movements were those of a conductor rather than a participator. After the last *vis-à-vis* steps, the man relieved the women of their hand pieces and led them down the street to their estufa. The choir followed, returning to the estufa in the same order it had started in. The whole performance had taken about half an hour. It was now the turn of the companion estufa, the estufa in the east end, north side, of "Middle Row." estufa A, I will call it.¹ It is the estufa which always

¹ A Zuñi informant has since called it *wa'kukaiahamia* (meaning on the east side. East in Keresan is *waha*), and he equated the estufa with *hekiapa kiwitsine* at Zuñi. Estufa B he called *gataa*, equating it with *heiva kiwitsine*. The other estufas were called *yachinguishlsee* (Zuñi, *chupawa kiwitsine*) *hachulsetsish* (Zuñi, *uplsanna kiwitsine*), *gutawe* (Zuñi, *ohewa kiwitsine*), *hemosh* (Zuñi, *muhewa kiwitsine*). The last

begins the dance, but this year its dancers were not ready, so word was sent to estufa B to begin. The performance of estufa A was in almost all respects a replica of that of estufa B. The most notable variation was the presence of the three "war chiefs" in the rear of the group, in a separate line. During the dance by estufa B they had been sitting with the other officers on a ledge of rock on the east side of the cross street. During the subsequent performances of both groups the war chiefs sat with the officers on this ledge. The group from estufa A proceeded westward down the street and then turning north through the cross street took up the same position on *kakati*. In the women's dancing there were slight variations, the patterns being just the same, but combined somewhat differently, and their execution was considerably superior. From now on until sunset the two groups performed alternately,¹ the drummer summoning his group from the estufa about the time the other group quitted the dance floor. Each group came out five times. Each is supposed to come out ten times, I was told, but it is never done because they start so late. The only variation in the performances was caused by the appearance, during the first part of the general dance, of women, one, two, three or more at a time, with pans or bowls of bread or meat which they threw into the dancing group. The men scrambled for the things without interrupting their song or dance. On the appearance of the women throwers the women dancers withdrew to the outskirts of the group—merely to preclude being hit, I was told, by the flying objects.

two the informant was naming from the dances' they presented, *gutuwé* meaning in Zuñi *watempla* (all kinds), the dance associated with the *ohewa kiwitsine*, and *hemosh* being identified with the Zuñi *hemishikwe*, the dance associated with *muhewa kiwitsine*. Incidentally the same informant mentioned the *hematatsi* (Zuñi, *upikajupona*), *waiyusha* (duck, Zuñi *muluktakya*), and *chakwe* (Zuñi, *chakwena*) as danced at Acoma.

Bandelier a'so mentions six estufas at Acoma, "Final Report," pt. 1, p. 268. *Papers of the Archaeological Institute of America, American Series*, iv.

I may add that my host at Acoma would not tell me the names of the estufas or the number. Estufa in Keresan is *ku'ch*. See p. 182, also Kroeber, A. L., "Zuñi Kin and Clan," p. 145, n. 1, *Anthropological Papers of the American Museum of Natural History*, vol. xviii, pt. II.

¹ Lummis describes a similar alternation in the tablet dance (*ayashtyucotz*) held at Cochiti in honor of the Catholic priest (*el baile del padre*) on July 14, the saints' day (*The Land of Poco Tiempo*, pp. 253-69. New York, 1897).

The men of the choir wore the usual miscellany of American clothes, wearing hats or bandas, boots or moccasins, overcoats or blankets. The three war chiefs were conspicuous from their red bandas and red blankets. The man from estufa B who beat time for the dancing women wore two downy eagle feathers in his hair, and across his cheekbones was a line of red paint. The women wore the usual women's moccasins (*kutsehashuwimi*, moccasins, *kutsi* = antelope)—the toes of one pair were stained magenta—and the usual combination of silk kerchief across the back (*pitone*, Zuñi, *utinats*, Acoma), black dress fastened over the right shoulder and under the left (*yatone*, Zuñi, *dyusits*, Acoma), and American cotton cloth, *i. e.*, the embroidered white petticoat showing under the *dyusits* and the calico garment above. The *dyusits* was fastened along the side with silver buttons or bits of tin. On their crown was a circlet of white, whether of paint or cotton or downy feather I could not distinguish. It was open towards the forehead where attached to the hair were two downy eagle feathers. A round spot of red paint almost covered each cheek. Their hands were painted white, the fingers laden with turquoise and silver rings. As I have noted, the feather paraphernalia carried or worn by the two sets of women varied. The head ornament of the two dancers consisted of a large bunch of parrot feathers and a few peacock which was held upright in place at the back of the head. From them fell two twists of reddish yarn to the ends of which at the waist line a smaller bunch of the same feathers was attached. The two stiff eagle feathers and the two bells and, in the case of the women of estufa A, the large yellow artificial flower the dancers held in each hand, were bound in a bunch of red yarn which showed above and below the clasping hand. The other two women, in the case of estufa A, held nothing in their hands, in the case of estufa B they held little commercial "dusters" of colored feathers.

The evening of this day two sets of dancers were going about town, dancing in several houses. The two houses to which we followed them were the house of Miller, the dance director, I presume, of estufa B, and a house where the governor was sitting. In both houses the audiences appeared to consist only of the house-

holds and their usual visitors. The first set of dancers consisted of eight young or middle-aged men performing a Navajo dance. They danced in line, stepping vigorously and singing to the drum. They wore American clothes but for a silk kerchief tied around the waist and pendant to it at the back a foxskin. They wore a banda and, standing up from it, on one side or the other, two stiff eagle feathers. Bells were attached below the right knee and in the right hand each dancer carried a gourd rattle. During this dance and the next, in pauses of the song, the audience uttered words of approval, an expression not customary in Zuñi. (Is it Mexican?) A few minutes after the Navajo dancers left and after the governor had asked me questions about the Zuñi *shalako*—for example, how tall the *shalako* mask was—a group of about fifteen boys came in to give a Comanche dance or to watch it. Three boys about seventeen sang, and another carried a bag for the gift expected from the household on leaving. In both houses the woman of the household dropped a roll of bread into the bag. There was but one dancer, a little fellow about ten years old. On his head and shoulders he wore the silk square which forms the woman's *utinats*. On the crown of his head was a crest of about ten stiff eagle feathers. In his right hand he carried a toy bow. Now and then some one in the audience smiled a bit at the boy's agile performance but on the whole the audience took the boys quite seriously, giving them exactly the same words of encouragement as they had given the older performers.

The next morning about 7.15 the church bell began to toll. Soon thereafter a few persons straggled into the church to pray;¹ but there was no general service. During the following hour I noticed men with bundles under their blankets ascending the ladder of estufa A. At 8.30 the drummer descended and beat his drum. The men descended, forming in two lines single file, with the three war chiefs in the rear and back of them a solitary figure, the "cacique." All² began to sing and to dance forward. After advancing about one hundred feet to the west the head of one line turned in

¹ On the morning of every dance, I was told, the church is open for prayer.

² The deaf old "cacique" did not sing, and his stepping was rather feeble. On his face was a pleased and curiously fatuous and absent look.

and back, the head of the other line keeping on the outside and also turning back. Meeting each other the two heads danced forward again, leading their lines to the dance floor, *kakati*. Here the same turning figure was repeated north and south. Then the two lines went up "North Row" to the east, repeating the figure, then down "North Row" to the west, repeating the figure, then back to the east. On this fourth repetition they threw out to the crowd from underneath their blankets ears of corn, turnips, boxes of crackers, and packages of tobacco. The boys and the women scrambled for the things and, closing in, began good-naturedly to mob the throwers. They persisted, however, in their dancing and singing and proceeded back to their *estufa*. The "cacique" had on an American overcoat and carried nothing to throw; but all the other men were blanketed, their blankets mostly red. Some of the men had irregular blotches of red, red and white, or white paint on their cheeks.

On the withdrawal of the group from *estufa* A the group from *estufa* B turned out, dancing four times east and west or west and east. They did not dance north and south on *kakati*. They too threw presents. They formed a smaller group than the *estufa* A group, numbering eighteen to the twenty-five or six of *estufa* A. The two heads, Miller and another, had their faces entirely whitened and both wore two downy eagle feathers in the hair. The performance of both groups lasted not more than a half hour. All the people in town, or at least the greater part of them, turned out to look on.

This morning the dancers came out about eleven. They followed the same routine as the day before. The only difference I noted was the dressing up of five men in the group from *estufa* B. They were decorated with knots of ribbons and fringes of colored paper and sashes of calico, a variegated and tasteless get-up. In their hair they wore eagle or parrot feathers. When the two women and their male leader withdrew from the group, these five bedizened men followed. With one exception the women's parts were filled by different women from the day before. On both days the women from *Estufa* A were middle-aged to old. Among the women of *estufa* B some were young.

Having to leave town about 4.30 p. m. unfortunately I did not see the conclusion of the dance. It concludes, I was told, with the coming together of the groups from the two estufas. So anxious was my host to have me go and so tricky about getting me off that I suspect the dance concluded with some particular ritual, perhaps with the little purificatory rite which concluded the corresponding dance at Zuñi, the *owinahāiye*. And yet that rite of waving a bit of cedar bark around the head four times and throwing it on the ground, that rite may well have been forgotten in Acoma together with the rest of the dance that had been forgotten, "the last old man who knew how they used to dance it" having died. That the dance used to be the war chief's dance to be danced on taking a scalp was still remembered.

The people were going to stay on at Acoma for another ten days or so, I was told by the governor. "They have to," he added, "we have not given them the rules yet." American citizenship, boundary questions and the introduction of a judiciary system appeared to be the political issues of the day. Having a judge in the American way has raised up an acculturation problem; for they feel that a judge would interfere, as he certainly would, with the functions of *tapup* and *teniente*.

These officers, we noted, are installed on December 30. They are installed after a week of mixed Catholic and native ceremonies. On December 24 there is *wispira* (vispera), *i. e.*, the church bell rings and the *kahera* goes drumming about town to summon the people to church the following morning. On December 25 all go to church. On December 26 and 27 there are Comanche and Navajo dances. On December 28 there is a church service. On December 29 there are Comanche and Navajo dances. The *kachale* come out and there is the *pasku* or "Butterfly" dance. During this time the "cacique" and his "brothers" and "uncles," *i. e.*, the younger and older members of the *kutsi* (Antelope) clan meet in *their* house.¹ There would be about ten of them, and the

¹ It is the house the "cacique" is said to frequent during the day; but whether it is merely the house of his people or a ceremonial house I do not know. I paid two calls on the "cacique," one in the evening, one in the morning. On both occasions he was in the house back of estufa A. The lower room was on a level with the

group is, I surmise, the *principales*. They discuss the men eligible for office and decide on nominees. On December 30 a meeting of all the men is called in the *komanira*, the long house standing near the church where the officers hold court. On this occasion the outgoing war chiefs announce the nominations. There is a rising vote, a merely formal vote, for everybody has to assent. If a nominee himself does not assent, he may leave and tell the "cacique." But the objection of a nominee is not heeded; once nominated, he has to take office. Instances are cited of men knowing in advance of their nomination running away. Even so, on their return, they were forced to take office.

In installing officers, the outgoing officer kneels on both knees, he makes the sign of the Cross, he says the "Mexican prayer," *rosa*, and he passes the cane on to his successor. All present kneel, removing, of course, their hats. Bandas are not removed.

Besides the officers, *tapup*, *tenientes*, and war chiefs, there are ten *principales*. They are chosen by the "cacique" from the Antelope clan. Their position is for life. Young men are eligible. They appear to act as a higher court. For example, were the federal agent to propose to the governor to establish a school at Acoma the governor would take the matter to the *principales*.

Obviously, the Antelope clan is the ruling Acoma clan. It supplies the "cacique" and the *principales* and through them it keeps control of the people. The present "cacique" has been in office since 1893. He is the fourth of four blood brothers to hold the office.¹ He is about eighty years old, rather feeble and quite deaf. "He knows nothing about the people," said his brother's son, my host.

estufa and between it and the estufa was a window about 3 feet by 4 feet, covered with cloth. In this general living room that evening were besides the "cacique" a little girl, a woman and a man. The house is said to be that of the wife of the "cacique." In the morning the upper room, whose floor is on a level with the roof of the estufa, was filled with persons going and coming—it was the second morning of the dance. The upper room and from it the lower have to be reached by crossing the roof of the estufa close to its hatchway.

¹ Stevenson writes in 1896 that the then *kiakwemosi lashi* was blood brother to his predecessor and that the son of this brother was his associate and destined successor ("The Zuni Indians," *Twenty-third Annual Report, Bureau of American Ethnology* (1901-2) p. 167). In 1916 two members of this priesthood were uncle and nephew, the third was unrelated. (Kroeber, p. 169.)

"He does not go about. People come to him. It is his brothers and uncles who go about and find out and tell him what to do. He takes their advice." They control the land distribution. Grazing land is communal, but agricultural land is held individually and application for it must be made to the "cacique." His harvest is brought in for him by the people.¹ They do not plant for him. Four rabbit hunts are held for him each year, one in each of the cardinal directions.² The hunt is held "after the war chiefs say they have been fasting four days and it is time to have a hunt." Some days after the rabbit hunt for the "cacique" there is a general hunt in which the women also take part.

The war chiefs have undoubtedly sacerdotal functions. They are said to pray morning and night and at ceremonials, to pray for the people, for their animals, for their crops, and for rain. Of these prayers or rites I could learn nothing definitely, nor could I learn anything of the sacerdotal functions of the "cacique." Of fraternities or curing orders I got merely a hint, a hint of the existence of two, the fire or flame and the flint societies.³ The existence of any group like the Zuñi *koyemshi* was denied,⁴ but of the group corresponding to the other Zuñi group of delight makers, the *newekwe*, I got some account. *Kachale* is their Acoma name. The faces of the *kachale* are painted white with black lines across the cheekbones.

¹ The Zuñi used to bring in the harvests of the paramount *ashiwanni* or priests including the *pekwin*. The custom lapsed at the death of the *pekwin* who died about thirty years ago.

Since writing the foregoing I have been told by one of the paramount *ashiwanni* that men used also to plant for them and women used to grind. The *apilashiwanni* would issue the orders. The *ashiwanni* had no say about land holding.

² The rabbits the Zuñi take in their quadrennial ceremonial hunt they give to the paramount *ashiwanni*. Game taken in the hunting before the quadrennial *kyanakwe* ceremonial also goes to the *ashiwanni*.

³ My Zuñi informant refers to the *chañani* of Acoma as the *hishdya* (knife, flint) corresponding to the Zuñi *halokwe* (ant, a subdivision is *achiakwe*, knife) and the *haken*, corresponding to the *malke lannakwe*, big fire fraternity of Zuñi. He also spoke of the *kupishlāiya* of Acoma as an organization and as *kwa teulhashi aho ahalikwe* (not amiable people witches). At Laguna the *kupishlāiya* are the cosmic gods.

⁴ My Zuni informant asserts that there are twelve *koyemshi* at Acoma. Their masks are bright red with eyes and mouth blue. They come out in all the masked dances.

They wear a "rag" circlet around the head with corn husk pokes attached to the hair. Their antics appear to correspond to those of the Zuñi *newekwe*. They "come out," I infer, only with the masked dancers. They appear to act also as sergeants-at-arms. "When they come for you, you must go with them. If you don't, they take away your clothes." They bring the boys in to the estufas.¹ There are twelve *kachale*, I was told. There was also a slight suggestion that the office was hereditary.

The Acoma clan is maternal and exogamous. A list of clans and of kinship terms is to be published in a later number of this journal. As at Zuñi, several kinship functions appear to be incumbent upon the father's clan. At birth, for example, it is the father's mother who washes the new-born child, and she it is, according to one informant, who gives the child its Indian name, taking it out before sunrise to the east side of the mesa. Again it is the father's people who come in after a death and wash the heads of the mourners in the household.

The Zuñi custom of lying in at childbirth on a hot sand bed is not found at Acoma. It is wholly unfamiliar. As I described the Zuñi practice to my host it seemed so odd to him that he appealed to his wife's kinswoman, the widow of a Zuñi, for corroboration. The practice being absent, the classification of beings into the cooked, *i. e.*, those who have thus come into the world, and the uncooked or raw, *i. e.*, animals, spirits or gods, and non-Zuñi humans, is also lacking. The Acoma mother lies in for four days. During this time an ear of corn is placed next to the child.² On the fourth morning, according to one informant, the "medicine-man" and his wife are sent for. They arrive with the "medicine water" some time before dawn. The "medicine-man" prays and sings four or five songs. He then takes the child out the east side of the mesa to show it at sunrise to the sun. The child's mother

¹ Nothing more could I learn about the initiation except that a boy joined the same estufa his blood father belonged to. He joined at twelve or fourteen. No girls joined. My Zuñi informant states that a sponsor or ceremonial father is chosen by parents and presented with meal. Not all boys are taken into the *kalsena tsashpč* (in Zuñi, *kolikyane*).

² Stevenson, M. C., "The Sia," *Eleventh Annual Report Bureau American Ethnology*, p. 140, (1889-90).

goes along and during the ceremonial sprinkles meal.¹ It is the "medicine-man" who proposes a name for the child. If his suggestion is not acceptable, any relative may name the child.

After the child is seven or eight months old the church christening is held. A *comadre* (godmother to your child) and a *compadre* (godfather to your child) are chosen; the child, if a girl, to take the name of the *comadre*, if a boy, that of the *compadre*. In the church the infant's head is wet with water and salt is presented to his lips. The godparents give the infant a present. They take it to their house and after characteristic Pueblo Indian fashion they wash its head—an interesting instance of the way the Catholic rite may be combined with a native practice.² Thereafter, every Christmas, presents are exchanged between the child and the godparents.³

The institution of Catholic sponsorship figures again at marriage. A man and his wife are chosen by the parents of either the bride or bridegroom to be "best man" and "best woman" (*suyena-kukuyenahiesishe*). After the wedding they take the bridal couple to their house, wash their heads and give them advice. The man talks to them first, then his wife. The man gives a present to the bridegroom, the woman, to the bride. The Catholic marriage appears to be firmly established in Acoma. The church service is indispensable, and there is no divorce. Whether or not as a consequence there is more adultery at Acoma than at Zuñi, *i. e.*, whether the lax system has succeeded the brittle system, I had no opportunity to observe.

The bridegroom gives a present to the bride—perhaps a dress.

¹ According to another informant the child's mother does not go along to the edge of the mesa. Cp. "The Sia," p. 141.

² At Zuñi the husband of a woman who has been present at the birth of the child becomes his ceremonial father, *i. e.*, his initiator into the *kolikyane*. Marriage into the family of the ceremonial father is taboo. May not this Zuñi custom have originated in the Catholic god-parent custom?

³ The godparent custom is, of course, thoroughly Spanish. It is a custom which seems to take hold on people. I have found it persistent among the Portuguese Negroes from the Cape Verde Islands living in New England. At Acoma, as among the Portuguese Negroes, the terms are applied loosely. At Acoma, for example, my contemporary in the household referred to me as *comadre*.

Beliefs of sympathetic magic are held in connection with conception and pregnancy and growth. To conceive a male, a woman will wash a bow and arrow and drink the water; to conceive a female, she will wash the grinding stone, and drink. A man or woman who eats corn whose grains grow in a double row or two chili growing together will beget or conceive twins. The remains of the lunch a hunter takes with him in the sacred rabbit hunts should not be eaten by a married woman or by the unmarried, otherwise they will have twins.¹ A pregnant woman will not eat rabbit. The skin on the belly of a rabbit is thin and her child's belly would be over large. A pregnant woman should not stand looking out of a window,² nor, once she has started to cross a threshold, should she return, for example, for anything forgotten. She should first cross the threshold and then return, otherwise the labor will be slow. To hasten labor, throughout her pregnancy a woman should rise quickly on awakening and put on her dress and belt, not dawdling about getting up. When it thunders a child will pull his hair to make it grow and stretch up his arms to grow tall.³ To make a backward or feeble child grow quickly and strong, a father will take it often out doors before the dawn.⁴

Burial takes place within the twenty-four hours after death. The dead are buried in the cemetery in front of the church. There is no division of the sexes in the cemetery, as in Zuñi, where the men are buried to the south of the central cross and the women to the north. At Acoma either the father's people or the mother's people take part in the burial. The possessions of the deceased are buried with him. The burial company is made up of men, although usually one woman goes along to carry a jar of water to break on the grave.⁵ The hair of the burial party is washed after the burial. For four days after death the spirit of the deceased

¹ Cf. Parsons, E. C., "Zuñi Conception and Pregnancy Beliefs," in *Proceedings of the Nineteenth International Congress of Americanists* (Washington, 1917), p. 381.

² *Ib.*, p. 382.

³ As a Zuñi child does at conclusion of a folk-tale.

⁴ There may be no element of sympathetic magic in this practice.

⁵ At Zuñi, according to Cushing, a water jar was broken by kinswomen inside the dwelling (*Century*, N. S. IV (1883), p. 47).

lingers about home. The house door is not left open, as at Zuñi, nor is trade taboo. After four days a "medicine-man" brings the prayer feather-sticks (*hachamani*)¹ he has made at home to the house of the deceased, places them where the deceased had lain, prays and bids the deceased begone. Then the "medicine-man" carries the feather-sticks to *shipapu koiyatum*, the "gate to *shipapu*,"² a place about one mile to the west of the town where the rocky conformation opens to the north.³ *Shipapu* is north, "north of Taos."⁴ Meanwhile the household drinks a cedar brew and vomits and the father's kindred come in and wash their heads.⁵ The heads of a widower and of his children are washed by the women of his mother's household. The widower's head has also been washed after the burial. Now, at the expiration of the four days,⁶ the widower may elect to return home with his mother or sister or to remain in his wife's household to look after his children. His children stay on in any event in their mother's household. After a year a widower or a widow may remarry. If one married sooner the families would not like it, besides the one remarrying might fall sick.

The morning after we arrived in Acoma the fifteen-year-old daughter of our host was delivered of a son. The girl's labor had lasted over two days and the child was still-born, the top of its head, and its arms and shoulders appearing, according to its grand-

¹ General name for prayer feather-sticks. "They take the breath," was said of the feather-sticks. After offering them at any time the same four days of continence observed at Zuñi seem to be observed at Acoma.

² The *katsina* or gods come, I was told, from *shipapu*. My informant asked me, by the way, not to mention to any one in Acoma that he had told me the word *katsina*.

³ Similarly it is at *akyia kaipa*, the place where the river widens and whence it bears offerings to *koluwela* that Zuñi feather-sticks are planted, but at Zuñi they are planted the day of the burial.

⁴ From my Zuñi informant I got a different account. The feather-sticks for the dead are cut on the third day, for a woman by the men of her household, for a man by the *katsena tsashpě*. Early on the fourth morning the feather-sticks for a woman are buried at the head of her grave in the cemetery, the feather-sticks for a man are taken down to *wenimatse i. e.*, to the *koluwela* below the mesa.

⁵ One informant stated that all the clanspeople wash their heads, the clanspeople of the deceased and his father's clanspeople.

⁶ At Zuñi as soon as the corpse is taken out, a widower's kinswomen come to take him to their home. He does not enter his wife's house again.

mother, to be "cut." The girl's father carried the body the same morning to the cemetery. While he was gone the girl died. We knew nothing of the birth or of the deaths until that afternoon. We had been given the upper room of the two-storied dwelling and the expectant mother lay below. They had not told us about her, because, I heard, they were "ashamed." The child was fatherless. It had been conceived while the girl and her parents were employed in California at the San Diego fair. Late that afternoon and until 7.30 in the evening there was a fairly continuous wailing in the death room and now and then quiet crying on the part of an elderly kinswoman who came up to our room to be with a child or to make bread or prepare a meal.¹ This sobbing appeared altogether spontaneous and was quite different from the crying of *ai he he* we heard from below.² We had sent word to the dead girl's mother asking if she would not like us to move to another house. She sent word back that she hoped we would stay, that we must not be "afraid" to stay. That evening she came up to our room and she described the deathbed scene, telling us how her daughter had said, "I love you, my mother. Be happy. I don't mind the baby dying." The girl sat up to eat. Soon after, to the utter incomprehension of her mother, she dropped back dead.

The following morning at dawn³ the wailing set in, lasting on

¹ Except in the more Americanized households meals are served in characteristic Pueblo fashion on the floor, members of the household sitting on boxes in a circle around the coffee pot, the bowls of stew and chili, and the little piles of bread. A male guest on leaving the circle says *woehe*, a female, *naicha* ("thanks").

² This same woman, the widow of the Zuñi, had both sobbed and wailed on our arrival. My Zuñi friend was a connection of her husband, who had died about a year before. It was because of that connection we were entertained in this house. The house belonged to the widow and her two elderly sisters. With them lived two married nieces, their husbands and children. The girl who died was the daughter of one of these nieces and the man I refer to as our host, he having taken the greater responsibility for our entertainment. The other man in the household was younger and he appeared to spend the larger part of his time walking the floor with a child two or three years old on his back. In a neighboring house another young father likewise played nurse. "You couldn't get Zuñi men to do that," said my friend. It surprised her more than it surprised me because in the Rio Grande pueblos and at Jemez I had seen the younger men carry the children in the same patient, tireless way.

³ At Zuñi this is the time an aged clanswoman of the father's clan sprinkles the corpse first with water from a gourd and then with meal.

and off until 9.30, half an hour after the corpse was carried out. The corpse was carried in a blanket,¹ a man at each of the four corners. The bearers were younger men. There followed in irregular order the girl's father, carrying a spade and pick, another man also carrying grave-digging tools,² her fourteen-year-old brother carrying her pallet and pillow, and a man, quite old, carrying in two flour bags over his shoulder what we took to be the girl's personal belongings. Her tatting spindle we had seen removed from our room a few minutes before they left the house. The company went to the cemetery by the southwest way, on the outskirts of the town. With them was no water carrier and when I observed the grave two days later no broken pottery was to be seen on it. On the northeast corner a candle was planted and at that hour—it was the time people were going to church—the candle was lit. The next day it had fallen over. (By this time the mourning period was past.) The candle stood at the head of the grave as the dead are buried, as at Zuñi, the head to the east.³ At home in our room for three nights after the burial the lamp was kept lit.⁴

The day of the burial sheep were being killed for the approaching feast and our household also killed and prepared a sheep. Its pelt was for sale and I saw the mother of the deceased bargaining with the American buyer who was going the rounds of the houses. There was cooking in the oven on the terrace and one of the old ladies made wafer bread on the slab in our room. On the stove was a brew of cedar twigs. Some of it must have been drunk the following morning by the mother of the deceased for I saw her vomiting large quantities of water from the terrace about 8.30. That morning the girl's father was out looking after his horses. The next

¹ This blanket had been contributed to the funeral by the Zuñi guests. They had taken the blanket with them from Zuñi to give on their departure to the household happening to entertain them in Acoma.

² The grave is not dug, as at Zuñi, in advance.

³ There is for this reason the same repugnance as at Zuñi to sleeping with the head to the east. My first night at Acoma I happened to ask the women to make up my pallet the top to the east and they thought it strange. Of course as soon as my attention was directed to it by my Zuñi friend I changed the position.

⁴ Contrary to Zuñi custom the door was not kept open.

morning he brought in on a burro a load of wood. In the evening he ate supper with me and took me to see his "uncle," the "cacique." This had been the first day of the *hoinawe* dance. None of the household went along the street to see the dance. In the evening, however, after our call on his "uncle," the "cacique," my host took me into two houses to see the Navajo and Comanche dances.

The second day of the dance, *i. e.*, the fifth day after the death, all the household had a hair wash. I had seen the yucca roots lying in my room, but the washing I did not see, as it took place in the room below. (The townspeople at large had washed their hair the day before, the first morning of the dance.) This day the younger woman of the household, the children, and our host watched the dance in the street, and one of the old ladies was among the women to throw bread to the dancers. Obviously the period of mourning had been concluded.

Fragmentary as were my observations at Acoma, at Laguna they were still more fragmentary, but here I was limited more from lack of time than from the suspicions of informants. I had one excellent elderly informant, the sister, blood sister,¹ of the priest who corresponds I take it to the "cacique" of Acoma, and for comparative purposes at least the information I got from her and from two or three other natives may be worth recording.

At death a medicine-man prepares a crown of *Muki* (Hopi) cotton for the head of the deceased. It is the medicine-man who sprinkles both corn pollen (*hatawe*) and meal² (*skatina*) on the corpse. He does not sprinkle it first with water.³ Over the cheek-

¹ Their mother's father was Kwime. He was the first Laguna, indeed his granddaughter alleged, the first Pueblo Indian, who could read. For seven years he had been educated in Durango, Mexico, by priests. He brought back with him a book containing "songs" which he "gave out." The book was kept under the *samahiye* ("idol") of his altar. After his death some one once wrapped the book up in the blanket of a deceased relative. It was discovered before the burial, however, and removed. Subsequently it disappeared.—It is a striking little instance of acculturation.

² He uses both, whether the deceased is male or female. Ordinarily, women use meal for ceremonial purposes, and men, pollen.

³ It occurs to me that these funerary rites may once have been limited to priests or to fraternity members. (Cf. "The Sia," pp. 67, 144.) But my informants did not so state. The yellow paste is said to make the deceased look life-like.

bones he smears a paste of ground yellow sandstone. A jar of water is broken on the grave. The dead are buried head to the east and there is a repugnance to sleeping in this position. Candles are kept lit in the place where the deceased had lain. Four days after the death the father's people come in to wash the hair of the mourners. All the clans-people wash their hair. The spirits of all the dead go to *wenimatse* (*matse*, blood) where the *katsena* come from.¹ It is a big lake somewhere near Zuñi.² The dead are fed elaborately on All Souls Day,³ but it is a daily practice to drop or crumble a bit of bread at each meal or to put it on the fire.

The customs of having godparents and of having wedding sponsors prevail. One informant who had been to Zuñi was very critical of the Zuñi marriage system. "They separate whenever they get tired of each other—after a week perhaps or even two or three days." And he added, "Here marriage is for life." There exists at Zuñi a custom of making friends (*kihe*) ceremonially. Two women, two men, or a man and woman may exchange gifts and each have his or her head washed by the relatives of the other.⁴ I doubt if this custom exists at Laguna.⁵ *Saukin* is Keresan for friend, and there is no special term for the friend made ceremonially.

The custom of a man adopting the life of a woman is known at Laguna. The adoption is optional on the part of a boy. He chooses to become a *kokwimu* merely because he prefers woman's work to man's. There is at present only one *kokwimu*.⁶ He is

¹ According to Fewkes "the Hopi believe that the breath body of the Zuñi goes to a sacred place near Saint Johns called Wénimá. There the dead are supposed to be changed into Katsinas, and the place is reputed to be one of the houses of these personages," *Twenty-fifth Annual Report Bureau of American Ethnology*, p. 312 (1893-4).

² This reference to the Zuñi *koluwela* came from my elderly woman informant. A younger informant insisted that "after we die we go up into the sky." It occurs to me that the Zuñi belief that the dead return in the clouds may have originated in the Catholic belief in heaven. At any rate it was certainly not denied by it.

³ Parsons, E. C., "All Souls' Day at Zuñi, Acoma and Laguna," *Journal of American Folk-Lore*, vol. xxx. pp. 495-6 (1917).

⁴ See Parsons, E. C., "Ceremonial Friendship at Zuñi," *American Anthropologist* (N. S.), vol. 19, no. 1 (1917), p. 1.

⁵ One Laguna informant had heard of it. Acoma informants knew nothing of it.

⁶ There is one (*kokwima*) also at Acoma.

about fifty years old and he became a *kokwimu* unusually late in life, when he was about thirty. His father objected to the change and the young man waited until after his father died. My elderly woman informant had known of eight *kokwimu* during her lifetime. She knew of no ceremonial functions performed by them, but she had heard of physical relations between them and men.

By one informant I was told there were four *estufas*, by another, only one, *dyup gama* (badger house), and that boys might join it at any time. Many boys do not join it. Of interest perhaps in this connection was the fact that on January 26 the *chakwena* was danced, but danced without masks because there was not an adequate number of the initiated who alone could wear the masks. In Zuñi the *chakwena* would be danced by members of the *estufas*, and I am interpreting the lack of men qualified to wear the masks from this standpoint. But there may be another explanation,¹ for the *chakwena* or "black masks"² as they are referred to at Laguna are spoken of as an "order." There are two orders of *katsena* or *gatsena*, they tell you, the black and the blue. The blue are subdivided into the *hemish*, the *kohashtoch* (feathers erect), the *kāiya* (all kinds).³ I had with me during my visit a copy of Stevenson's *The Zuñi Indians* and I asked my elderly woman informant to show me the pictures of those masked personages she had known at Laguna. Those once seen at Laguna, but seen no longer are:

kolama, pl. LIV.

hehea (Laguna *hea*) pls. LII, LIII.

Those still seen are:

sayalia, pl. XVI (in Laguna also he whips with yucca switches).⁴

¹ There is another explanation, I have since learned. The *dyup gama* referred to is merely the dwelling of the *dyup* man, *hash surni* (old man Zuñi), who is custodian of the *katsena* masks and director of the *katsena*. There are no *estufas* at Laguna. Rooms are borrowed for ceremonial purposes.

² *Shutsmasewi* is another name for *chakwena*.

³ The Zuñi dance the *chakwena*, which they state is borrowed from Laguna, and the *toichakwena* (home made *chakwena*). *Hemish* may correspond to the Zuñi *hemishikwe* and *kāiya* to the Zuñi *watempla* (all kinds, i. e., a miscellany of *koko* or *katsena*). Since writing the above I find that a Zuñi ex-resident of Laguna affirmed to Dr. Kroeber such correspondence. ("Zuñi Kin and Clan," p. 145, n. 1.)

⁴ *Saiahlia* is a *Sia katsuna* which figures at the initiation as at Zuñi ("The Sia," p. 117).

kwelele, pl. XXVII.

koyemshi, pl. LXVI (Laguna *kumeyoish*).

salymobia, pls. LVI, LVII (Laguna, *salolobia*).

uwannami (as the *kokokshi* dancers are miscalled), pl. LXX.

natashku, pl. LXXI ("something like it").

kolowisi, pls. XIII, XIV seemed familiar as *shuwi* (snake) *katsena*.

I learned of another mask at Laguna—*chapio*—which corresponds quite closely to a Zuñi figure—*atoshle*.¹ The mask of *chapio* is black with white round eyes, a long tongue, and white hair. He wears ordinary American clothes. He carries a long whip. He comes out "when snow falls" or when he is sent for by a parent to berate a naughty child and make it promise to "mind." He has been known to rope a child and drag it across the river. He is given bread by the household. He is said to come from the south—from El Paso.

At Laguna we also find the Zuñi *koyemshi*—*kumeyoish*. Anyone may play the part at the bidding of *hash surni*. The *kumeyoish* come out with the *katsena*. As at Zuñi, they play running and beanbag games to amuse the people. Their blankets (*manta*), masks, etc., are just like the Zuñi make-up. In the five knobs of their masks are coral, white shell, turquoise, meal, and corn pollen.

The winter solstice ceremonial must differ considerably from the Zuñi ceremonial. For example the rite of throwing out fire appears to be unknown, likewise that of taking out refuse.² (The one cleaning up time which occurs at Laguna is before the Saint's dance, the dance of San José on September 19.) A mixed blood, Margaret Marmon Eckerman, the daughter of the woman I refer to as my elderly woman informant, told me that the winter and summer solstice dances, due she supposed December 22 and June 24, were determined with much uncertainty at Laguna. During the past year they had been danced on December 7 and on June 7 and 8. The people used to rely on the *osach* (sun) or *kurena cheani* to

¹ See Parsons, E. C., "The Zuñi *adoshle* and *suuke*," *American Anthropologist* (July-Sept., 1916). By my Zuñi informant *chapio* was identified with *atoshle*.

² Unknown too at Acoma, according to my Zuñi informant. For eight days a fire is kept up in the *estufa*, and watched by the *huchañani*. None might get a light from it for his cigarette.

determine the dates.¹ They now rely, according to one informant, on the *shiwanna cheani*. According to another informant, it is the war chiefs who take the observations. A high rocky hill about four miles to the southeast of Laguna, known as *osach gama* (sun house),² is the place they observe both for the winter solstice, *kuwamishugu* (south corner) and for the summer solstice, *diamishugu* (north corner).

One informant told me there used to be six *cheani*, another, that there had been very many. There are at present at Laguna only two *cheani*, *osach cheani* and *shiwanna*³ *cheani*. Thirty or forty years ago there was a considerable migration to Isleta led or caused by a *hakani* (fire) *cheani*. He took the woman of another man with him to Isleta and his kindred followed. The *hish* (flint) *cheani* went too. What became of another *cheani* I heard of, the *saiyap cheani*, I do not know.⁴ The *samahiye* of the *saiyap cheani* was from the sand hills of the south. It was painted like a *katsena*. Besides *samahiye*, *cheani* have *iyetik*, a corn cob wrapped in feathers (Zuñi *mili*). The *iyetik* are set in a row on the altar. *Osach cheani* is referred to not only as the head of the *cheani*, but of the whole sacerdotal system. He is being overlooked, however. For example, at the *katsena* dances they fail to send for him as they are supposed to. He continues to offer prayer feather-sticks (*hachamuni*), however, for the winter and summer solstice, offering near the river. All the *cheani* used to offer feather-sticks. Formerly after a four-day retreat they made an annual summer pilgrimage to Mt. Taylor, the highest peak in the conspicuous mountain range twenty miles

¹ He kept a calendar by marking on a house wall.

² Cf. *tawaki* (sun house) of the Hopi (Fewkes, J. W., in *American Anthropologist*, vol. XI, (1899) p. 69).

³ Meaning fire or lightning. Persons shocked by lightning are eligible. See Parsons, E. C., "Zuñi Inoculative Magic," *Science*, N.S., vol. XLIX (1916) p. 470. When a Laguna is struck by lightning he may not be touched until he revives of himself. The Lightning Fraternity Stevenson mentions at Zuñi may have been copied from Laguna.

⁴ As equivalent for the Zuñi *matke lanna* fraternity the Laguna ex-resident gave Dr. Kroeber the Laguna *hakani*. For the Zuñi *shiwanakwe* he gave the Laguna *shiwana*, and for the *sayapa* masks in the Zuñi *shuma'kwe*, he gave the Laguna *sayapa* (*saiyap*). He also mentioned the Laguna *hishtianni* knife. ("Zuñi Kin and Clan," p. 145, n. 1.)

northwest of Laguna and the highest mountain peak in New Mexico. Nowadays the pilgrimage is made only in time of drought. There is on Mt. Taylor a big hole called *shiwanna gacheti* (lightning home). To it lead four well-marked trails, one from Laguna, one from Taos, Santa Clara, etc., one from Acoma, one from Zuñi. Cloture of the hole is the cause of drought, and so the *cheani* open it and offer feather-sticks. A few years ago after they had offered their sticks on Mt. Taylor in a period of drought, before their return to Laguna there was a heavy downpour of rain. On Mt. Taylor the *cheani* also find herbs for their medicines. The *cheani* are medicine-men as well as rain makers.¹ In making a cure a *cheani* would go out at night entirely nude to look for the heart of the invalid. He might dive down into the river. At any rate on his return home he appears exhausted and he goes into a trance. He has to be rubbed down by his relatives and whatever his condition they may not weep over him because it would delay his recovery. Any one may become a *cheani*, it was said, just as any one may become a *kachale* or delight-maker.

The *kachale* were "sent" to make mirth, to lighten people's burdens. They were not to doctor, but against the approval of the *cheani*,² they took to doctoring. Like the Zuñi *newekwe* they use the offal of animals as medicine.³ If you are cured by a *kachale* you sent for while he was out playing, you must join the order. *Kachale* practise jugglery (*waiishgenai*).

I referred to the war chiefs taking solstice observations. They appear to have other sacerdotal functions.⁴ They offer feather-

¹ The two Zuñi institutions of *ashiwanni* (rain priests) and *tikyawe* (fraternities) seem undifferentiated at Laguna. This may be due in part to the disintegration at Laguna of ceremonialism. At Sia, too, rain-making seems to be a fraternity function.

² The *kachale* appear to have encroached in other ways too upon the *cheani*. In the somewhat bitter criticism of the *kachale* by my elderly woman informant, the sister, I recall, of *osach cheani*, I had a glimpse, I thought, of one of those institutional feuds apparently characteristic of Pueblo Indian society.

³ Margaret M. Eckerman once saw a *kachale* visiting from a Rio Grande pueblo eat dung in the plaza. She has never seen a Laguna *kachale* do this.

⁴ Asked for the equivalent of the Zuñi *ashiwanni* the Laguna ex-resident named what appears to be the war chiefs (*tša'tauhu'tcani*) ("Zuñi Kin and Clan," p. 145, n. 1.) My own Zuñi informant said positively that neither at Laguna or Acoma was there anyone corresponding to the *pekwin* of Zuñi and he more definitely identified the war chiefs with the *apilashiwanni*.

sticks four times each month—in each quarter of the moon.¹ The three war chiefs (*tsatïohuche*) are elected annually with the governor (*tapup*) and the two *tenientes* (*hamashutseiche teniente* and *châitseiche* (younger) *teniente*). The council (*tsewileme*) of all the men² is always held on January first. The outlying Laguna settlements or colonies make nominations and all present vote. It is a free vote, it is claimed, *i. e.*, it is undictated.³ At the installation the outgoing governor hands the cane to the *shiwanna cheani* and he hands it with a prayer to the incoming governor.

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¹ I could learn nothing of laymen offering feather-sticks; but prayers are said at sunrise and meal sprinkled. Meal is sprinkled too in connection with deer and rabbit hunting. After a deer is killed he is laid out with his head toward the home of his hunter. After the deer is carried home he is laid indifferently, but here beads, a woman's belt and bells are placed on top of him. The women then sprinkle him with meal. (Cf. "The Sia," p. 120.) Dead rabbits they also sprinkle with meal. And when a rabbit is put on the fire two or three grains of corn are put with it. This rite is not observed for a deer.

² There appears to be no age qualification. Youths of any age may attend.

³ There appears indeed to be no such clan control as at Acoma. The *kumeyoish* origin myth points to the existence in former days of a dominant clan, and of the same clan that dominates Acoma, the *kuuls hanooh* (Antelope clan). The fact that today an aged *kuuls* clansman is always consulted on the *kalsena* dances and that *kuuls* clansmen would take their position at the head of the line of dancers may be vestiges of the ancient clan domination.

NOTES ON THE HANDS AND FEET OF AMERICAN NATIVES

BY H. F. C. TEN KATE

THE present article was originally intended to form part of a series of papers which I published, and which are further going to appear in *L'Anthropologie* under the general title of "Mélanges anthropologiques." The great difficulties and unavoidable tardiness of publication of these memoirs, however, in consequence of the war, induces me to present the following article to the *American Anthropologist*, although in a somewhat different form than first intended, and as I did previously with another contribution in relation to the "Mélanges."¹ Herewith the work, of which the first number appeared in 1913, comes at last to an end.

The following data pertaining to hands and feet of North and South American Indians, Carbugres and Bush Negroes, both natives of Surinam, are based upon my own observations and measurements, mostly made and taken in the field, mainly in the years 1883, 1885-86, and 1888. With the exception of a few figures concerning the stature and the indices of hands and feet, all these data are here published for the first time.

I am well aware of the scantiness of my material as it is given here, but it should be studied in connection with the other somatological data which, as I said before, have been or are to be published elsewhere and of which it forms the necessary supplement.² Besides measurements and observations on hands and feet of exotic races, as treated in the present paper, as far as I know, are comparatively

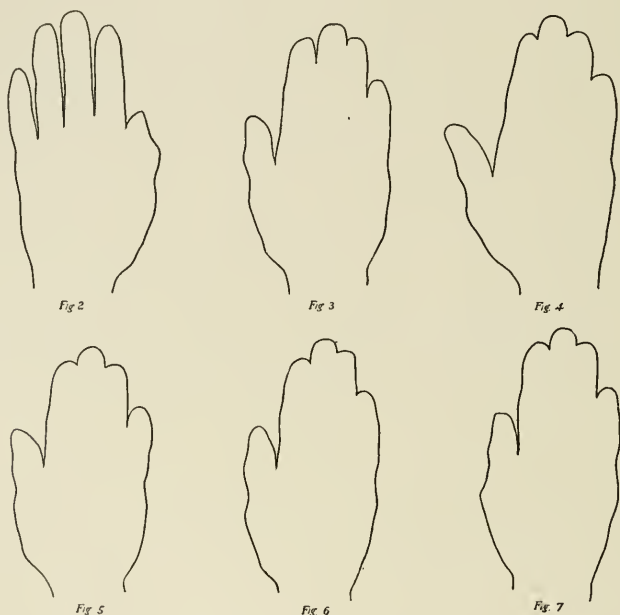
¹ "Dynamometric Observations on Various Peoples," *American Anthropologist* (N. S.), vol. 18 (1916).

² See for the North American Indians in particular my "Somatological Observations on Indians of the Southwest," *Journal of American Ethnology and Archaeology*, vol. III (Boston, 1892), and "Mélanges anthropologiques," no. VI; for the natives of Guayana, *Revue d'Anthropologie* (Paris, 1887), pp. 44 seq. and *Encyclopaedie van Nederlandsch West-Indië* (The Hague and Leiden, 1914-17), pp. 113-116, 165-166; for the Guayaqui Indians of Paraguay, *Anales del Museo de La Plata, Seccion antropologica*, vol. II (La Plata, 1897).

rare, and our knowledge of this subject is apparently still rather fragmentary. Hence my venture to publish these notes for what they are worth.

The present study is chiefly based upon the outlines, on paper, of 44 hands and 42 feet, belonging to 54 subjects, namely 31 North American Indians, 18 South American Indians, and 5 Carbugres and Bush Negroes. Moreover I obtained the length of the fingers of 6 Navajo and 18 Zuñi Indians by direct measurement with the *compas glissière*.

All these outlines were drawn, according to the instructions of



FIGS. 2-7.—Tracings of hands ; size $\frac{3}{16}$.

French anthropologists, with a pencil specially adapted for this purpose (*crayon approprié*). On the outlines thus obtained, the measurements were taken.¹ To avoid misunderstanding, the following, however, should be remembered. The transversal diameter of the hand is the metacarpo-digital line and at the same time the greatest breadth of the hand. The longitudinal diameter

¹ Cf. Topinard, *Eléments d'Anthropologie générale*, pp. 1135-1136.

of the foot represents the maximum length, regardless whether the first or the second toe is the longest. Thus this measure is frequently one of projection. The transversal diameter of the foot follows the metatarso-phalangean line, and gives at the same time the greatest breadth of the foot. Moreover it should be borne in mind that as a rule there is a slight difference between the measurements taken on the outlines and those taken directly on the living subjects. This refers in particular to the diameters of length of hands and feet. I think that on the whole the figures resulting from outline measurements are the most reliable of the two methods. My measurements were mostly taken with the *compas glissière*, except when the feet were too long for this instrument. In such cases I measured the greatest length with the *glissière anthropométrique* of Topinard, constructed by Collin. The figures of absolute measurements represent millimeters. Nearly all the anatomical and morphological details mentioned in the succeeding tables and discussed here, are inferred from the outline drawings. The accompanying illustrations are selected from my collection of outline drawings and more or less representative of the different hand and foot types found among the subjects under discussion.

All these subjects are apparently normal. The great majority consists of adults, presumably between twenty and forty-five years of age. There are four or five cases presumably from sixteen to nineteen years among them, and only one old subject. With the exception of three cases of very young subjects, approximately from ten to fifteen years, all the others are included in the averages, as far as these have been calculated.

I have divided my material into three groups, each of which will be discussed separately. Finally an attempt at comparison between these groups has been made as far as feasible with the scanty material at my disposal.

NORTH AMERICAN INDIANS

From Table I it will be seen that on the average the index of hands and feet is higher among the males than among the females; in other words the former have larger hands than the latter.

TABLE I
MALES

No.	Tribe	Right or Left	Stature	Hands				Remarks	Feet				Re- marks				
				Greatest		Index	Length		Breadth	Longest Toe	Interstices Be- tween Toes						
				Length	Breadth		in Rela- tion to Stature = 100				in Rela- tion to Stature = 100	i/ii		ii/iii	iii/iv		
1	Maricopa....	L	1.786	198	90	45.4	11.0	5.0	Fig. 2	L	270	105	44.0	15.6	5.8	i-ii	
2	Mohave....	R	201	89	44.2		L	257	104	40.4	i	
3	".....	R	1.850	210	87	41.4	11.4	4.7		L	270	110	40.7	14.6	5.9	i	Fig. 8
4	Chemehuevi.	R	197	81	41.1		L	267	111	41.5	i	Fig. 9
5	".....			L	262	107	41.9	i	
6	Comanche....	R	190	85	44.7									
7	Yaqui.....	L	1.695	187	84	44.9	11.0	4.9	Approximate stature	R	249	104	41.7	14.6	6.1	ii	
8	".....			R	266	102	38.3	i	
9	".....			R	262	104	39.6	ii	Fig. 10
10	".....			R	248	113	45.1	i	Fig. 11
11	Pima.....	R	1.730	182	85	46.7	10.5	4.9									
12	".....	R	1.640	181	82	45.3	11.0	5.0									
13	".....	L	1.830	212	90	42.3	11.5	4.9									
14	".....										
15	".....	R	1.720	189	83	43.9	10.9	4.8		R	270	100	39.9	ii	
16	".....	R	1.700	201	78	38.8	11.8	4.5									
17	".....	R	1.712	204	80	39.2	11.9	4.6									
18	".....	R	1.754	199	76	38.0	11.3	4.3									
19	".....	R	1.640	182	82	45.0	11.0	5.0	Fig. 3	R	258	96	37.2	14.7	6.7	i	
20	".....	R	1.662	192	87	45.3	11.5	5.2	Fig. 4	R	247	96	38.8	14.4	5.9	i	Fig. 12
21	Papago.....	L	183	78	42.6		R	250	105	42.0	15.6	6.3	i	
22	".....										
23	".....	L	184	76	41.3								i	
General average		1.743	193.7	83.1	42.6	11.2	4.6	Approximate stature	...	257.4	104.2	41.1	14.9	6.1		

TABLE I—Continued
FEMALES

No.	Tribe	Hands							Feet											
		Right or Left	Stature	Greatest		Index	Length	Breadth	Remarks	Right or Left	Greatest		Index	Length	Breadth	Longest Toe	Interstices Between Toes			Re- marks
				Length	Breadth						Length	Breadth					11/11	11/111	111/111	
1	Maricopa....	R	1,600	179	74	41.3	11.1	4.6	R	254	95	37.3	15.9	5.9	I
2	Pima.....	R	1,562	183	74	40.4	11.0	4.6	L	236	93	39.4	15.1	5.9	I
3	".....	R	1,610	179	74	41.3	11.1	4.5	R	231	96	41.1	14.3	5.9	I
4	".....	L	1,598	186	80	44.3	11.1	5.0	L	244	100	40.8	14.6	6.2	I
5	Papago.....	R	183	76	41.5	Aged	L	241	93	38.6	I	Fig. 13
6	".....	R	184	79	42.9	R	241	93	38.6	I
7	".....	R	176	74	42.0
General average	1,592	181.4	75.8	41.9	11.0	4.6	241.2	95.4	39.4	14.9	5.9
8	Chemehuevi.	R	170	76	44.7	Approximate age 14-15

The ratio of the hand length and hand breadth to the stature (= 100) is practically the same in both sexes. As for the centesimal ratio of the feet measures to stature, there is only a difference in the relative breadth of the feet, which is larger among the males.

As could be expected, as a rule in both sexes the fourth finger is longer than the second or forefinger, but in two cases in twenty-five, or 8 per cent., both these fingers are of equal length. In two other cases, or equally 8 per cent., the forefinger is longer than the fourth finger. In this connection I can mention that the twenty-four Navajo and Zuñi Indians of both sexes, which I measured at Fort Defiance and Pueblo de Zuñi, present the following dispositions: second and fourth finger of equal length in two cases, or 8.3 per cent.; second finger longer than fourth in six cases, or 25 per cent.

The average absolute length of the three middle fingers of said Indians is as follows:

	II, Millimeters	III, Millimeters	IV, Millimeters
5 male Navajo.....	89.8	101.2	94.2
10 male Zuñi.....	84.7	94.3	87.2
8 female Zuñi.....	79.6	88.2	83.1

The length of these fingers of a Navajo woman is respectively 91, 98, and 87 mm.

Taking all the North American Indians of the different tribes together, we find that the average difference in length between the second and fourth finger is 4.6 mm. for the males and 2.3 for the females. The greatest individual difference, as presented by a Mohave, attained 13 mm.

Even a superficial inspection of the outlines of the hands and feet shows that there are morphologically two principal types or forms among them, besides one or two forms which might be called intermediate or mixed forms; and of course the individual variations. Thus it is easy to distinguish a long and narrow, or slender, and a short and broad, or heavy type. This applies of course also to the South American Indians and other natives.

The fingers belonging to the first-mentioned type are comparatively thin and well formed; those of the second type thick and

clumsy. It seems that the thumb of the first type, in relation to the other fingers, is longer than the thumb of the second type, where it is sometimes strikingly short and heavy.

The distribution of the various hand forms among the Indians of the above table is as follows:

25 Hands	17 Males	8 Females	Total Percentage
Long.....	11	7	72
Short.....	2	—	8
Intermediate.....	4	1	20

As regards the feet, as a rule the first toe is the longest. In one case, or 5.2 per cent., the first and second toe are of equal length. The second toe exceeds the first in three cases, or 15.7 per cent.

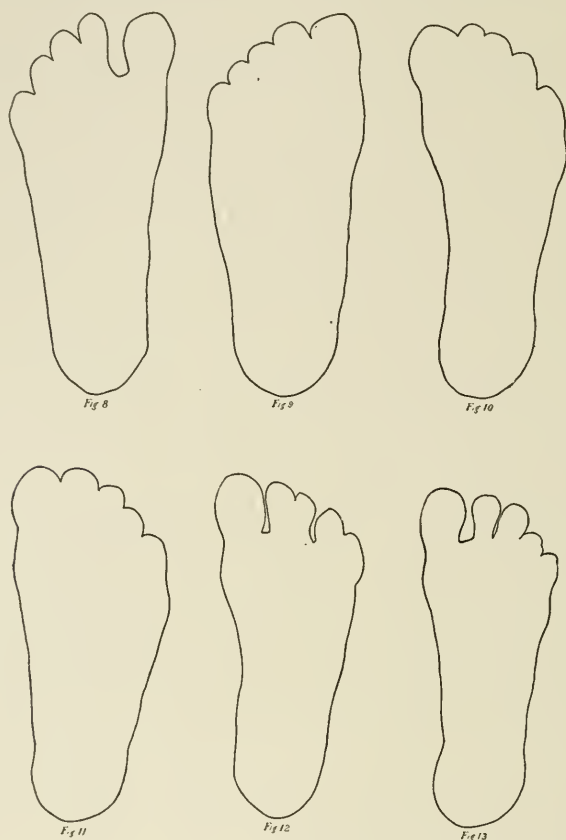
The great or first toe tends outward in four cases, or 21 per cent., among which only once in a more or less marked degree. In two cases, or 10.5 per cent., there is a deviation of the toes, specially marked in the third to fifth. These toes, however, while they are more or less spread out like a fan, point outward, contrary to what usually happens in such cases.

In two cases, or 10.5 per cent., a marked concave incurvation or bent of the inward margin of the foot occurs. This incurvation is situated between the metatarso-phalangean line and the calcanean portion of the foot. In both cases the first toe is more or less tended outward. This peculiar configuration of the inward outline and part of the sole, is especially marked in the foot of a Yaqui and of a Pima Indian (figs. 10 and 12). It frequently has been observed, as well by myself as by others, among Malays and Indonesians, and occurs also to a certain degree in the so-called *carrasco*-foot of the natives of the Philippine Islands.

Interstices between the different toes occur in nine individual cases, or 47.3 per cent. In three of these cases different interstices are found on the same foot.

Morphologically, the differences of the feet are more marked than those of the hands. This greater difference lies not only in the variety of configuration of the inward margin of the foot and of the disposition of the toes, but also in the difference in breadth

of the metatarso-phalangean or anterior portion and that of the calcanean or posterior part of the foot. In the long and narrow type this difference is as a rule comparatively slight, while it is generally more marked in the short and broad type. For obvious



FIGS. 8-13.—Tracings of feet ; size $1\frac{3}{5}$.

reasons this difference in breadth is caused that the foot index, or centesimal ratio between maximum length and breadth, does not necessarily express the type of foot. In some cases, however, a certain degree of correlation between the two exists.

The principal forms of feet among the North American Indians are thus distributed:

19 Feet	14 Males	5 Females	Total Percentage
Long.....	9	4	68.4
Short.....	2	—	10.5
Intermediate.....	3	1	21.0

SOUTH AMERICAN INDIANS

The main conclusions we can draw from Table II are the following.

The hand and foot index of the males is considerably higher than that of the females; in other words the former have larger hands and feet than the latter.

The length and breadth of the hand and the length of the foot in relation to the stature (= 100) is nearly the same in both sexes, while the breadth of the foot of the males exceeds that of the females. With the exception of one case in fourteen, 7.1 per cent., the fourth finger in both sexes is always longer than the second.

The average difference in length of the second and fourth finger is 4.2 mm. among the males and 3.5 among the females.

What I have said before with reference to the relative shortness of the thumb applies more particularly to the South American Indians. At least those of the broad hand type have a very short thumb.

As for the different forms of hand, their distribution is as follows:

14 hands	8 Males	6 Females	Total Percentage
Long.....	2	4	42.8
Short.....	2	2	28.5
Intermediate.....	4	—	28.5

In the majority of cases the first toe is the longest. In two cases, 11.1 per cent., the first and second toe are of the same length. In four cases, or 22.2 per cent., the second toe is the longest.

The great toe tends outward in eleven cases, or 61.1 per cent., six of which, though strongly marked, never reach the degree which is found in the *carrasco*-foot and among the Negritos. Neither is the fan-like spreading or tending sideward of the other toes so striking, although it occurs in two cases, 11.1 per cent.

The concave incurvation, mentioned among my North American subjects, occurs three times, or 17.2 per cent., among the South American Indians (*cf.* fig. 14).

The interstices between the different toes are generally much more marked and wider than among the North American Indians. The trapeziform interstice observed by Maurel¹ among the Galibis, which also is much less frequent and less developed among the Indians of North America, occurs in several cases. Figure 15 (the foot of a male Arawak of Epira on the Corentyn river) shows this curious disposition in a marked degree. Interstices generally are found in fifteen cases, or 83.3 per cent., among the Indians of the above table.

The following figures show the distribution of the different forms of feet:

18 Feet	11 Males	7 Females	Total Percentage
Long.....	—	2	11.1
Short.....	7	2	50.0
Intermediate.....	4	3	38.8

The greater prevalence, by 39.5 per cent., of the short and broad foot type among the South American Indians is only partly confirmed by the average foot index. What I have said before also applies to these Indians, and in a more marked degree. Their most frequent or typical foot has a very large metatarso-phalangean diameter in comparison with the breadth of the calcanean portion, situated behind the bi-malleolar line. Roughly speaking, seen in outline from above, this type of foot has more or less the form of a blunt wedge, the point of which is represented by the heel. Figures 15 and 16, representing the feet of an Arawak and a Warrau, are fair specimens of this type, with which the foot of a Yaqui Indian (fig. 11) can be compared.

CARBUGRES AND BUSH NEGROES

The Indian Carbugres or Carbugre Indians of Surinam are a crossbreed between Indians and Negroes. Their physical characteristics generally resemble more those of the full-blood Indians than those of the Negroes. My three subjects are of Carib-Negro extraction.

¹ Cf. *Revue d'Anthropologie* (1887), p. 53.

The general aspect of the Bush Negroes is truly Nigritic-African notwithstanding they have inhabited Surinam for generations.

No. 1 of the Table III belongs to the Beku-Musinga tribe; No. 2 to that of the Yucas.

In view of the very small number of subjects, my comment on Table III can be brief.

If we may judge from these three cases of Carbugres, their



FIGS. 14-21.—Tracings of feet; size $\frac{3}{16}$.

hands are on the average comparatively large according to the index, and both long and large in their centesimal relation to the stature.

The index of the foot is on the whole a trifle higher than that of the male Guayana Indians. Their relative breadth is the same as in my series of full-blood Indians.

TABLE III
CARBUGRES

No.	Sex.	Hands						Feet														
		Right or Left	Stature	Greatest		Index	Length		Breadth	Remarks	Right or Left	Greatest		Index	Length		Breadth	Longest Toe	Interstices Between Toes			Remarks
				Length	Breadth		Length	Breadth				Length	Breadth		1/i	ii/iii			iii/iv			
1	Male.....	L	1,540	189	82	43.3	12.2	5.3	L	239	91	38.0	15.5	5.9
2	".....	L	1,470	172	75	43.6	11.7	5.1	L	231	102	44.1	15.7	7.2
3	Female.....	R	1,360	155	72	46.4	11.3	5.2	Approximate age 12-14	L	208	81	38.9	15.2	5.9	Fig. 19
BUSH NEGROES																						
1	Female.....	R	1,525	181	73	40.3	11.2	4.7	Fig. 6	R	235	87	37.0	15.4	5.7	Fig. 20
2	".....	R	1,500	183	76	41.5	12.2	4.9	Alleged age 16	L	240	88	36.6	16.0	5.8	Fig. 21
										Fig. 7							IV/V	

In one case the second toe is the longest. An interstice between the first and second toe occurs in two cases; the concave incurvation of the foot in two cases.

An outstanding first toe, together with a more or less fan-like spreading of the third and fifth toes, occurs once. Two hands and one foot belong to the broad type; one hand and two feet pertain to the elongated form. As for the Bush Negro females, it will suffice to say that their hands, but specially their feet, and these latter in a marked degree, are long and narrow. This is not only evidenced by the index and the centesimal ratio to stature, but morphologically also by their long and narrow type. In other words, both show characteristics which are found in certain groups of African Negroes.

SUMMARY OF THE PRINCIPAL GENERAL RESULTS

Scanty as the preceding data are, I think we are nevertheless justified in drawing certain conclusions which, of course, have only a relative value, and should not necessarily lead to generalizations. I shall refrain from comparisons with other races, as far as this would be possible with the material brought together in the well-known handbooks of Topinard and Rudolf Martin and with the observations of a few field anthropologists in the Indian Archipelago, further Oceania and elsewhere.

Although there is an enormous difference in the absolute measurements or dimensions of the North and South American Indians of both sexes, the relative measurements present on the whole only very slight differences.

The greatest differences, although not considerable, lie in the hand and foot indices of the male and female North and South American Indians. In other words, the hands and feet of the latter are mostly larger, both according to the indices as well as in relation to stature ($= 100$). This fact is fully confirmed by the relative prevalence of short, broad hands and feet among them; the North American Indians having proportionately longer and slenderer hands and feet.

The metatarso-phalangeal or anterior portion in relation to the

calcaneal or posterior part is very broad and comparatively much larger among the South American Indians. These hand and foot types are doubtless in correlation with the height of body or stature, the North American Indians being on the whole much taller than the South American Indians. As for the other differences, metrical and morphological, of the two groups, there is generally more difference in the mutual length of the fingers, more particularly of the second and fourth, among the North American Indians.

The second toe exceeds the first more frequently in length among the South American Indians.

The outward deviation of the first toe, the more or less fan-like disposition of the other toes, and the interstices between them are also more frequent among the South American Indians, as well as the concave incurvation of the foot.

The hands and feet of the Indian Carbugres resemble in most respects those of the full-blood Indians.

The difference in the disposition of the toes, including the interstices, can perhaps, at least partly, be explained by two main factors, footgear and mode of life. The Indians of the Southwest and of Northern Mexico do not always go bare-footed, but they frequently wear sandals or, like the Chemehuevi and Paiute, also moccasins. The ground they tread is mostly composed of sandy plains or barren rocks. Besides the narrow dugout canoes of tropical South America are unknown to them. Except on Colorado river canoes are never used, and even there they are not very common.

The natives of tropical South America on the contrary have no footgear whatever. The forest trails are very narrow; oftener still there are no trails at all, and when the Indians go through the dense forest, they move slowly, with difficulty. But, what is more important, they pass a part of their existence in canoes. The mode of living and the surroundings of the South American natives, at least in the tropical regions, are in many respects very similar to those of the primitive natives, brown or black, of the Malay Peninsula, the Indian Archipelago, the Philippines, and further Oceania, where the same characteristics of the feet frequently occur.

As to the general muscular development of the hands in both sexes, it would seem that on the whole, and in relation to the stature, it is about equal among the North and South American Indians, notwithstanding the difference in pressure as revealed by the dynamometer.¹ Both groups and both sexes have hands developed by and used to constant exercise, either by handling various weapons and paddles, or by tilling the soil.

The muscular development of the feet, in correlation with that of the legs, however, generally seems to be in favor of the North American Indians, males and females. This discrepancy is probably partly due to the fact that, as I said before, many South American Indians pass a part of their life in canoes, which prevents the full development of the lower limbs. Furthermore foot races, ball and other games, in which these limbs are exercised, so common among the tribes of the Southwest, are unusual or unknown among the forest Indians of South America.²

ASHIYA, JAPAN

¹ Cf. "Dynamometric Observations," *loc. cit.*

² The above remarks on muscular development are not only based on the present material but also on numerous observations made in the field.

BOOK REVIEWS

METHODS AND PRINCIPLES

The American Indian: An Introduction to the Anthropology of the New World. CLARK WISSLER. Douglas C. McMurtrie, New York, 1917.

Except for Brinton's *American Race*, this book is the first attempt to treat of the anthropology of North and South America together in a manner more than a compilatory one. The comparison with Brinton's book is in one sense unfair to Dr. Wissler. Not only have the available data increased greatly in the past twenty-five years, but Dr. Wissler has made a definite attempt to utilize all of them where Brinton only sketched or selected along the bent of his personal interests. The true resemblance to the earlier author's work lies in the fact that both have consistently viewed the two continents of the New World as a single unit and succeeded in keeping themselves free from the all too prevalent tendency to think of the peoples and cultures of Mexico and Peru as if they were one subject of study, and those of the less civilized regions as another.

How far Dr. Wissler's ambitious attempt will meet a need which the public feels, or imagines that it feels, will not be considered here; although the satisfaction of this need may well prove to be the greatest achievement of his work. The book is in any event of extreme importance in the history of anthropology; and will be discussed in that light.

Two prefatory statements by the author characterize the scope. First, his review of the native cultures and peoples of the New World is intended to serve as an introduction to the methods and viewpoint of anthropology generally. Second, his ideal has been to treat fundamental problems rather than to present a digest of all possible phases of native life.

Great skill is shown by his beginning with a consideration of food, and thereby sketching the areas of culture, as related to their environments, without any formal definitions. This plan allows of the orderly review of data through the twelve following chapters, the several types or provinces of culture emerging more and more clearly while the treatment remains free. In the same way the preponderating influence of the civilizations of the middle parts of America is more and more illumined and reinforced without the need of any direct demonstration. A whole series of maps drives this point home again and again, besides making

possible the assemblage of enormous masses of data. In this way are treated agriculture, tobacco, animal transportation, basketry, weaving, dress, footwear, pottery, decorative designs, sculpture, and clan organization. In practically every case the distribution on the map, even if unaccompanied by text, would suffice to give some considerable glimpses into history.

Nearly eight chapters are devoted to the material aspects of culture, a little more than one to art and knowledge, two to society, and two to forms of religion. This may seem a somewhat disproportionate division, but is on the whole a fortunate one. The author's personal experience has been most largely in the tangible phases of culture, a subject which he has for a number of years handled with distinction and made in certain ways peculiarly his own. His concrete mode of treatment, which is one of the particular merits of the book, is also more successfully applicable—at least in the present state of knowledge—to material than to social and religious culture. It is exceedingly doubtful, for instance, whether a map of ceremonial systems could have been constructed which would show as much as any of the maps mentioned. Finally, the author has as his clear aim historical reconstruction, and this end is of course better served by consideration of the basic and fundamental elements that lie close to the origins of culture than by examination of its more or less evanescent non-material efflorescences.

A new part of the book begins with chapter XIV, in which the culture areas that have so far been only indirectly recognized are formally defined and their cultural content enumerated. Here we have a series of compact masses of cultural elements, listed in passages that are scarcely readable but are of reference value. The following chapter presents the archaeological data in parallel manner; and a third takes up the subject of the chronology of cultures. This is a rather brief section confined to some reviews of data and previous endeavors, and to rather inconclusive hints. Dr. Wissler's method makes too predominant a use of geographical factors to allow him to press very far the few available indications of a directly temporal character; and his procedure in this matter is undoubtedly wise, because consistent with his method.

Two farther chapters are devoted to language and somatology. The treatment accorded language is slight. Not only philological problems as such are avoided, but also problems whose bearings would be distinctly ethnological although their method must remain a linguistic one: for instance, the question of the causes and rate of speech differentiation, of ultimate or at least continental origins, of unity or diversity of

inner type irrespective of origin. There can be no doubt that when even partial answers to these problems are rendered, they will yield enormous insights into the history of American culture. At the same time, the first attack on these points should come from linguistic students, just as their final disposal must be by linguists dealing with linguistic evidence. Dr. Wissler can therefore scarcely be criticized for evincing in these matters a caution that amounts to avoidance and that stands out in contrast with his spirited initiative in attacking other problems. The close ultimate relation which exists between linguistics and ethnology, with the latter as the chief gainer from the efforts of the former, is illustrated by the extent and depth of influence which the Powell classification of linguistic stocks has from the day of its promulgation exercised on every aspect of American ethnology. This family relationship of languages—which has recently again been brought to anthropological attention through an attempt to reconsider the evidence—is the only linguistic problem seriously touched on by Dr. Wissler. His attitude on the question of a further reduction of stocks may be described as sympathetic in spirit and guarded in fact.

The chapter on somatology reveals a similar attitude but is much stronger and fuller. There is no attempt to consider the causes of differentiation of physical types, but the types themselves, their groupings and their origins, are definitely inquired into. It is true that the proposed regional groupings cannot be accepted as fully established, and that the discussion of origins is also tentative. It must be borne in mind however that constructive syntheses of data in the physical anthropology of America have heretofore been rather conspicuously wanting. Considering this deficiency, and the briefness of treatment necessarily imposed on Dr. Wissler, the conclusions appeal as concrete, soundly arrived at, and definitely pointing out the way to further possibilities. They are clearly a step ahead.

Nevertheless, neither the sections on language nor on physical types can begin to compare in volume or detail of data or fineness of treatment with those devoted to culture; which fact is mentioned as a reminder of how far American linguistics and somatology have lagged behind American ethnology in achievement.

In chapter XIX, the author reaches the kernel of his work: the correlations. He shows that his ethnological and archaeological classifications coincide to a remarkable degree, the only notable discrepancies being attributable to the disturbances of culture following the introduction of the maize complex, that is, agriculture. When this doubled cultural

classification is matched against the linguistic and somatological ones, the correlation is much weaker. Most modern anthropologists would have been so convinced of an absence of correlation that they would not have considered the matter at all, or would have contented themselves with pointing out a few striking instances of its failure. But Dr. Wissler is not afraid of fundamentals, nor does he hesitate to reexamine issues which tradition considers disposed of. His findings are best stated in his own words (p. 334):

We can, therefore, safely summarize our discussion by stating that each distinct culture area tends to have distinctive characters in language and somatology. However, the reversal of this formula does not hold, for people speaking languages of the same stock do not show a tendency to common culture characters unless they occupy a single geographical area. An analogous negation holds for somatology. It seems, then, that culture is one of the primary factors in this association, and that, due to causes we have not yet perceived, both languages and somatologies are differentiated after culture's own pattern.

On the question of the influence of migration he sums up (p. 336):

We are thus brought to the conclusion that the phenomena of our subject manifest a strong tendency to expand to the limits of the geographical area in which they arise, and no farther. Language and blood seem to spill over the edges far more readily than culture, from which we must infer that their dispersion is a by-product of migration, but that these migratory groups seem unable to resist complete cultural assimilation.

His treatment of the environmental factor is typical of a temperamental quality that pervades the entire book and which can perhaps best be characterized as one of liberality. Dr. Wissler's practical handling of environment would satisfy the requirements of the most ultra-modern methodology of cultural history. He does not once step even indirectly into the many tempting pitfalls of endeavoring to derive cultural content out of environment. On the other hand environment is never excluded from his consideration, in fact is kept in constant relation. There are few books, even among those of the most recent years, that so consistently attempt to show how culture makes use of its environment and how this utilization reacts on the culture itself. It is true that the outcome must be described as a recognition of tendencies rather than a formulation of principles; but these tendencies would probably be universally subscribed to. Again it is best to quote the conclusion (p. 340-1):

It appears a fair assumption that so long as the main sustaining habit-complexes of life remain the same in an area, there will be little change in material culture. This may be in part an explanation for the lack of close correspondence

between the historic cultures and archæology in the several parts of the great maize areas in contrast to identity elsewhere. The bison, salmon, wild acorn, and guanaco must have been in their respective habitats for a long, long time, and a culture once developed around them could be displaced only by a radical change, such as the introduction of agriculture or pastoral arts. Now the regions where maize was found in use at the opening of the historic period are just those in which archæology shows the most disparity. It seems then, that the environment as a static factor conserves the types of culture and because of this weighting of one of our three great groups of characters, breaks their unity, so that the same language and likewise the same blood may be found in association with different cultures according to the laws of historic accident.

There follows a chapter on the methods or mechanics of culture origin. This is really a theoretical discussion, with the American data used in illustration, and reapplications of the conclusions to the American field. Such findings are (p. 346):

The diffusion of material complexes has been by wholes. It was not merely a plant, a food, or an idea that was borrowed, but a complete method, with all of its associates. When however we turn to ceremonial practices and art, the case is less simple, for there seems to be a conflict between tribal patterns and the new trait.

It is no doubt this feeling that has contributed to induce Dr. Wissler to throw his main efforts on those simple cultural phases that yielded most generously under his treatment.

Again (p. 352):

We are left with a presumption that there are no direct functional relations between the several trait-complexes constituting a culture.

As regards the historical versus the biological conception of culture, the author makes it clear that he is not in the least tainted by any confusion. When however the statement follows (p. 353) that

anthropology is something more than the study of culture, it is essentially a coördinating and synthesizing science

we are in a position to realize the combination of sound method in detail with wide openmindedness—to some it may seem almost indecisive—which, with an unusual facility of assimilation of large masses of data, constitutes perhaps the most definite quality of the book.

The twenty-first and final chapter, on New World origins, concludes with a summary—sketched in outline in two scant pages—of the history of man and his culture in America. The author calls this a "hypothetical statement," but it may be presumed that his professional colleagues on

the whole will be more convinced of the essential soundness of his "theory" than of its practical utility. As a reconstruction it lacks the specific detail that makes a reconstruction impressive.

The Appendix of Linguistic Stocks, by Miss Bella Weitzner, is more than the name purports, being in fact a much needed table of American tribes. The bibliography is not a general one of the subject, but a list of references used.

It remains to comment on a few of the qualities of individuality displayed throughout the book. It is clear that finesses are deliberately eliminated. Separate facts are always suppressed, no matter how significant they may be aesthetically or emotionally, unless they definitely coordinate with considerable groups of other facts. Herein lies a possible danger to the appeal of the work to the wider public. It is not that the book is technical. Considering its range, it is marvelously free from technical terms and professional considerations. But it is a distinctly scientific book, without literary endeavor or flavor, or even serious inclination in that direction. And it may be questioned how large a hold on public interest any work may normally obtain which is not first of all a book and only incidentally a piece of science. *The American Indian* is distinctly a successful endeavor to be a piece of science.

Then, the book is much less the work of a craftsman who loves his material and his tools, than of a conceiver of undertakings, ready to extract value only where most profitable, and to scrap them instantly if yield can be increased. Dr. Wissler has looked almost wholly for mass results; and he has got them in a degree that makes all previous efforts in the same direction seem feebly puny. In the power of practical organization displayed, the book is characteristically American.

The author evinces remarkable balance of judgment. Always cautious, he is never timid; ever enterprising, he does not once become reckless. He looks into everything and faces any aspect impartially; if his conclusion comes out tentative, he is willing to have it so. He steers a mean course, equally clear from the Scylla of mere depiction and the Charybdis of theory spinning. The evolutionist, migrationist, and other snares that so regularly enmesh those with a weakness for deduction, never touch him. It is a pleasure to feel his apparently instinctive aversion from anything but inductive inference. He may be in error in many of his conclusions. He would no doubt be willing to be proved wrong in every conclusion, if thereby his science moved forward. He has moved it. The book traverses a long route; and there is scarcely a point touched but something is established which before was vague or obscure or postponed or unorganized.

Finally, the undertaking has one great import. The method of grouping culture elements and cultures by areas—or “centers” as Dr. Wissler likes to call them—and of tracing distributions, is not new. At least half of the anthropologists of this country have been reared in an atmosphere over which the concept of the culture area hovered insistently. But familiar as the concept was, it was somehow never applied consistently, as part of a larger scheme; and hence the virtues and inherent efficiency of the concept as a tool have never before been so clearly manifest, or been so productive of broad results, of actual historical reconstructions of subjects on which directly historical data are almost lacking. In England, the culture area concept has been almost wholly ignored, except now and then as a matter of form; and on the Continent, in spite of some half-systematic beginnings, it has tended to be neglected rather than utilized. If however the method which employs the culture area and the culture center is practicable for the attainment of large scale historical ends at all, there is no reason why it should be limited to America. It should be applicable with equal success to the African, Oceanic, and even Asiatic and early European fields. Here lies a promise—and a significant challenge. If this method proves useless against Old World phenomena, its value in the field of the New World and with it the value of this book at once become questionable. But if the systematic prosecution of this method, as best exemplified to date by Dr. Wissler's work, is genuinely productive, the entire science of anthropology, or at any rate its cultural portion, bids fair to be put on a new basis.

A. L. KROEBER

AMERICA

Ancient Civilizations of Mexico and Central America. HERBERT J. SPINDEN. (American Museum of Natural History, Handbook Series, no. 3, 238 pp.) New York, 1917.

This work from the hand of a leading authority should fill a long-felt want in the literature of Middle American archaeology: namely that for a popular, concise, and general presentation of the subject.

The purely technical treatise, even where the material is as rich and varied as here, possesses little interest for the general reader. He is soon lost in a maze of hypotheses, speculations, comparisons and expositions, wherein the very language used is not his own. The terminology is strange; the subject-matter, unfamiliar; and he wearies of the effort before his interest is aroused.

The present volume is especially designed to meet his particular needs. It is essentially a book for the general reader; and as such is

another step forward in the great educational program, which the American Museum has added to its many other activities, namely the bringing home of science, its aims and accomplishments, to the people.

First of all then, Doctor Spinden's book is designed for the general public. The intricate subject with which he deals has been stripped of all needless technicalities and the story told in the language of the layman for lay comprehension. The treatment is concise. The descriptions are brief and to the point. A vast range of subjects has been skillfully condensed, boiled down as it were, and the essential residua have been arranged in a clear and logical sequence.

Finally a general presentation of the many and intricate problems involved has been achieved for the first time. There is an admirable selection of material throughout. Details are everywhere subordinated to general features; and major rather than minor cultural tendencies and developments have been emphasized.

In arranging the subject-matter Doctor Spinden has followed the chronological method. The book opens with a brief introduction "designed to put before the reader such facts as may be necessary for a ready understanding of discussions and explanations that will follow."

The first chapter deals with the Archaic Period, the earliest cultural horizon of which we have definite knowledge in the New World. It is, of course, necessary to postulate a still earlier epoch, called by Doctor Spinden the Pre-Archaic Horizon, during which the New World was peopled from the Old. This is generally assumed to have taken place in Neolithic times from Asia by peoples who enjoyed only the simplest arts; stone-chipping, basketry, and fire-making. The non-perishable artifacts of such peoples, however, would appear to have been almost too crude and casual to permit their identification as such; and Doctor Spinden rightly leaves this moot and general question to one side, and passes to the second or Archaic Horizon for his opening chapter.

It is here, moreover, where his greatest contributions have been made. He was possibly the first, and certainly the most successful in describing this great cultural epoch; in establishing the characteristics of its art; and in defining its extension; and from a scientific point of view this chapter is probably the most valuable in the book.

It was with the invention of agriculture, probably somewhere on the highlands of Mexico, that the Archaic culture had its origin, Doctor Spinden believes; and it was with the extension of agriculture that it had its first tremendous dissemination, roughly from the Canadian line to Peru on the west coast of South America and to the mouth of the Amazon on the east coast.

In this chapter he lays a logical cultural foundation for the later florescence of our two leading native civilizations: the Maya and the Inca,¹ which took place more or less synchronously about the beginning of the Christian Era or slightly earlier, the former on the Atlantic coast plains of southern Mexico and northern Central America, and the latter on the highlands of Peru and Ecuador.

The second chapter of the book deals with the Maya, who, judged by and large, would appear to have been the more considerable of the two. Doctor Spinden adds little new material here to his already important contributions in this field, the present review of the subject being little more than a restatement of old facts.

He takes issue (p. 127) with the writer, over the latter's thesis that the Maya stelae were erected primarily to mark the passage of time: "Some writers have assumed that the stelae and other inscribed monuments were primarily time markers set up at the end of hotun (or five-year) periods. This seems an unnecessarily narrow view."

As so stated, this view is obviously too narrow to fit the observed facts, since the earliest monument known, Stela 9 at Uaxactun, as well as the four next earliest in the same region, Stelae 3, 10, and 17 at Tikal and Stela 3 at Uaxactun do not record hotun endings, *i. e.*, 8.14.10.13.15, 9.2.13.0.0, 9.3.6.2.0, 9.6.10.8.15, and 9.3.13.0.0 respectively.

The earliest hotun marker yet found is the recently discovered Stela 24 at Copan recording the very early date 9.2.10.0.0. It would seem from the limited evidence available therefore that the custom of erecting stelae on the hotun endings may have originated in the south, rather than in the north where it does not appear, so far as the writer is aware, until 125 years later, *i. e.*, 9.8.15.0.0.²

A more exact statement of the writer's position would therefore be the following: Primarily the Maya monuments were erected to mark the passage of time. At first they were probably erected at any time during the hotun, as for example the five stelae noted above; but as early as the second or third katun of Cycle 9 the custom of setting them up on the hotun endings was already practised in the south, though it may not have extended to the northern cities until a later date.

The summary of Mayan History given on pp. 130-135 differs but little from that proposed by the writer in a paper read before the Nine-

¹ With the Inca is here included that complex of earlier cultures, from whom the former inherited the larger elements of their civilization.

² Stela 25 at Piedras Negras records this latter hotun ending.

teenth International Congress of Americanists.¹ The creation of a Protohistoric Period prior to 9.0.0.0.0 appears superfluous, and is unnecessary, since the Early Period can be extended so as to cover the same time equally well. The creation of such a period is, in fact, away from, rather than toward that simplicity in classification, which Doctor Spinden usually achieves, and it were well eliminated.

The writer adds a Colonization Period for the New Empire, roughly contemporaneous with the Great Period of the Old Empire, during which Yucatan was discovered and occupied. The four closing periods of Doctor Spinden's summary correspond closely with the four closing periods of the writer's.

The third chapter deals with the Lesser Civilizations: the Zapotecan, Totonacan, Toltecan, Chorotegan and others, both north and south, which borrowed the more striking elements of their civilizations from the Maya, and which did not rise to prominence until after the fall of the Old Empire about 600 A.D.

These later cultures have been much less closely studied than the Maya, and are correspondingly less known. In this chapter Doctor Spinden attempts little more than a review of their more obvious features, opening up at the same time, however, many new and promising lines for future investigation.

As the matter now rests, much more is known of Middle America, particularly the region occupied by the Maya, during the first six centuries of the Christian Era than the succeeding six, when the lesser civilizations had their sway; and light here, to borrow an analogy from Egyptology, will tend to illumine "The Hyksos Period" of ancient American history.

The closing chapter treats of the Aztecs, the dominant people of Middle America at the time of the Spanish Conquest. Here again there is very little new material, but rather a careful selection from the large body of existing knowledge.

The Aztecs have probably been more written about than any other people of ancient America. The material here from both the native and Spanish sources is voluminous and varied, and care in selection as well as skill in condensation was again necessary in order to give a general picture in a limited space.

If there be one adverse criticism that might be leveled against the work

¹ "The Rise and Fall of the Maya Civilization in the Light of the Monuments and the Native Chronicles," *Proceedings of the Nineteenth International Congress of Americanists*, Washington, December, 1915, p. 140.

as a whole, it is the inadequacy of its conclusion. At the risk of enlarging the handbook by a few additional pages, it seems to the writer, this section could have been advantageously expanded to include a more detailed discussion of the sequence and interrelationships of the four culture horizons so clearly established, and some passing reference at least, made to the great civilizations of South America, the Inca, Chimu, and Proto Chimu, etc., and the points of their probable historical correlation.

The purely mechanical part of the volume leaves little to be desired. The size is convenient as becomes a handbook; the type and printing are clear; and the illustrations, 45 plates and 81 figures, are happily chosen and excellently reproduced. In short this latest number of the series is a convenient and valuable contribution to the literature of the subject, and will, as the writer has already ventured to predict, fill a long-felt want for a short general work on Middle American archaeology.

SYLVANUS GRISWOLD MORLEY

An Anthropological Study of the Origin of Eskimo Culture. H. P. STEENSBY. (Reprint from *Meddelelser om Grønland*, vol. LIII, pp. 228, 1 plate, 1 map.) København, 1916.

This paper is an elaboration of the author's point of view first definitely formalized in 1905 under the title "Om Eskimokulturens Oprindelse." It is not, however, a mere English translation of the former, but a deeper, more exhaustive, study. It is not my purpose to review the entire discussion, but to call attention to the main conclusions. As the title suggests, Steensby proposes to find out where and how the historic Eskimo culture came into existence. His discussion opens with an excellent summary of the views of others, a contribution that will be welcomed by university students. In particular, the presentation of Rink's views and method will be of the greatest service.

The current theories of Eskimo origins present but two alternatives as to place, Bering sea and Hudson bay. Among the supporters of the former are Crantz, Rink, and Thalbitzer; for the latter, Murdoch, Brinton, and Boas. Steensby, the author of the paper before us, takes his stand with the latter. All of the aforementioned, without exception, recognized that the most typical Eskimo culture centered around Hudson bay, they differ only in their interpretations of this fact. To one, the center of a culture is its place of origin; to the other, it is merely the place where it had the best chance to survive. The purpose of our author is to show that the evidence for a central origin is too strong to be ignored and further

that this culture grew up here as an intrusive unit intermediate to the Mackenzie area, on the one hand, and the Plains area, on the other. Even Rink made a strong case for an inland origin, though he chose the Yukon delta as the place where contact with the Arctic sea set up the reactions that produced Eskimo culture. It is almost certain, that had he at hand the data now available, he also would have selected a central area as the place of origin, at least such would be the natural outcome of his argument.

Steensby seeks his main proof in correlations between geographical conditions and Eskimo traits. He takes as his point of departure the two somewhat antithetic cycles of Eskimo life: the winter upon the smooth sea ice and the summer inland hunting for caribou and other game. This distinction was clearly made by Boas who until recently had the whole central field to himself, as Steensby states. He then discusses the geographical conditions of the whole Arctic coast, deciding that the region about Coronation gulf, or the Arctic archipelago, is the only one where the ice conditions, the aquatic mammals, and the caribou herds are adequate. While it is clear that this is the region in which one sees Eskimo life at its best, when regarded as an adjustment to Arctic conditions, one may be pardoned for being skeptical, since the argument brings us back to the starting point: namely, that here is where the culture is most typical. All this may be true and still not necessitate its being the place where the first experiments in Eskimo culture were made. Steensby, however, considers other data. If, for example, we have evidence for an expansion of the Eskimo from this center, then there will be no hesitancy on our part. The relatively recent expansion of Eskimo into Greenland is not open to debate, but that would also be in keeping with an Alaskan origin. On the west, he is only able to cite the opinions of Boas and Jochelson that in Alaska, Eskimo culture is an intrusive wedge between Siberia and America. He follows this up, however, by an attempt to show that marginal variations in Eskimo houses, kayaks, etc., are either adaptations of central traits or intrusions from Asia. From the reader's point of view, these are not always successful, for though our author is a master of geographical subjects, he does not seem exactly at home in the details of material culture, particularly when dealing with the work of master hands. Yet, on the whole, the case is a good one for an expansion of Eskimo culture from the Arctic archipelago. To our mind, *this* is the kind of data that will decide the question at issue, rather than mere circumstantial geographical evidence.

We come now to the most original contribution of Steensby, the

theory that a barren ground tribe moved to the adjacent coast and solved the problem of existence on the winter ice. He discusses at length the traits of the Mackenzie area and then of the Plains to show that Eskimo summer culture is essentially the same. This is, to our mind, the weakest part of the paper because the author does not show a strong grasp of the facts of Indian life and his citations indicate that he is not familiar with the recent work of American students. The parallel he points out is plain, but to explain it will need a keen analysis of several culture areas. Upon this parallel, as he sees it, the author concludes that:

The Palæeskimo culture was an original North Indian form of culture, the winter side of which had become espically and strongly developed by adaptation to the winter ice of the Arctic Ocean (p. 186).

As the case is left, however, we fail to see why it is not equally probable that the Eskimo took over these Indian traits at some early period of contact. It is one thing to prove that Eskimo culture was once inland, but quite another to derive it from a specific contemporaneous inland culture. The chronological factor enters here. When Thalbitzer argues for a western origin, he has in mind the antecedents of Central Eskimo culture and would no doubt grant that its present typical form took shape in the Arctic archipelago. This does not seem to have been clear to Steensby. Even the indications of recent Eskimo expansions to the west may be merely a "back-kick" into the region of their origin. On the other hand, Steensby is to be praised for his clear vision of the interrelations of culture, language, and blood, for he states more than once that he deals only with movements of culture and that Eskimo blood and language may have originated elsewhere and gone otherwise.

Finally, these remarks should be taken as a very inadequate notice of a most suggestive and stimulating paper. Every student of general cultural problems should read it.

CLARK WISSLER

Implements and Artefacts of the North-east Greenlanders. Finds from Graves and Settlements. THOMAS THOMSEN. Danmark-Ekspeditionen til Grønlands Nordøstkyst, 1906-1908. Bind IV, Nr. 5 (Reprint from Meddelelser om Grønland, vol. XLIV). Pp. 360-496, pls. VII-XXVI. København, 1917.

This publication is a concise but exhaustive review of the archaeological collections from Northeast Greenland, based chiefly upon the work of the Danmark Expedition of 1906-1908, but considers all the known objects from this region. The most northerly group of Eastern

Eskimo known to history is at Ammassalik, East Greenland, so well described by Thalbitzer and reviewed in this Journal (vol. 18, no. 1, Jan.-Mar. 1916, p. 115). Yet, north of this, in what is known as Northeast Greenland, traces of a former Eskimo population have been met with as far up as Peary Land. On the west side, traces of people have been noted by Peary and others to 82° North Latitude. These points of farthest north fall on opposite sides of the isthmus to Peary Land, hence it is natural to assume that the trail is continuous. The facts are, then, that the Eskimo have skirted the entire coast of Greenland.

From the very first, it was evident that the objects brought back from Northeast Greenland were nearer Central Eskimo culture than that of South Greenland. Boas was perhaps the first to suggest that the extinct Northeast Greenlanders came around the north of Greenland. Thalbitzer and other Danish writers have taken the same view.

With this in mind the author of this paper has made an intense comparative study of the collections from Northeast Greenland. His findings support the above theory at every point. They even go further and indicate that the route of migration was a direct one from the Arctic archipelago. This suggests that the Smith Sound, or Polar Eskimo, are a straggling remnant of those engaged in this movement.

The body of the paper is taken up with good technical accounts of the various artifacts and as such will be of the greatest service as a work of reference.

CLARK WISSLER

The Washo Indians. S. A. BARRETT. (Bulletin of the Public Museum of the City of Milwaukee, vol. 2, no. 1, pp. 1-52, pls. 1-13.) Milwaukee, 1917.

The Washo are one of those American tribes, like the Tonkawa and Walapai, whose name is familiar to everyone, although practically nothing is known of them. They should be of particular interest because they have long been reckoned as constituting an independent and isolated stock; and also because, perched on the east slope of the Sierra Nevada, they possess Plateau outlook and historical affiliations, while the basis of their culture appears to be Californian. Two sets of influences have therefore undoubtedly shaped their ethnic life, and the unraveling of these promises to be as interesting as important.

The present paper is a sketch of a collection of specimens with native terminology and a few collateral notes taken in the field; but slight as it

is, will prove welcome under the circumstances. The richness of Washo basketry development is an undoubted central Californian trait. The coiled ware especially approximates that of the Miwok, although of superior technical quality. The twining is much closer to that of the Plateau Shoshoneans, although similar forms of presumable Great Basin origin have long since become established among most of the Sierra tribes. Other manufactured objects discussed do not differ appreciably from recorded Californian types; but the poverty of published records on the material culture of the Northern and Southern Paiute makes it difficult to say whether the Washo resemblances to central California are local or part of more widely diffused distributions.

The resumption by the Milwaukee Museum of its series of publications is a sign of activity on which the institution is to be congratulated. It must be noted, however, that the illustrations are not of the quality expected in a descriptive paper in this age of mechanical excellence of pictures.

A. L. KROEBER

PHILIPPINE ISLANDS

A Study of Bagobo Ceremonial, Magic and Myth. LAURA WATSON BENEDICT. (New York Academy of Sciences). New York, 1916.

The New York Academy of Sciences is to be congratulated on the publication of this excellent study, dealing with the Bagobo, a pagan tribe of the southern Philippines. The author spent nearly two years investigating the culture of this people—first in the coast settlements, which have been somewhat modified by contact with Moro, Spanish, and American settlers; and later in the isolated mountain villages of the Talun district—and has gathered a wealth of material of interest to all students of primitive peoples. She has crowded a great amount of this information into the present volume, yet it is evident that she has only just begun to draw on a seemingly endless store.

To give an adequate idea of this study in a short review is quite impossible, yet a few notes may serve to convince the reader that he cannot afford to neglect a careful reading of the whole volume. Following a short description of the present condition of the Bagobo, and the conditions under which the work was carried on, the author introduces her subject with a statement of the general characteristics of the religious attitude of the people. This is characterized by the highly sacrificial nature of the ceremonials, by the composite make-up of the rites, in which are blended both offerings of the blood of slain victims and agricultural products; and by the non-esoteric character of the religious

life of the community. Of fundamental importance are the group assemblages at which sacrifices of human beings or fowls are presented to certain gods; sacred liquor is ceremonially drunk; formal lustrations in the river for the expulsion of disease take place; rites magically protective against ghosts and demons are manipulated; and material wealth in garments, ornaments, and weapons is offered up with the primary intention of obtaining an increase of riches. Yet it is noteworthy that the parents of every family, at their own home altar, are accustomed to perform devotions and to make offerings for the health and well-being of members of their household. Formal worship is carried on at fixed altars or at temporary shrines of recognized types, where fruits of the field and manufactured products are placed, or the slain victim is ceremonially offered up. But acceptable devotions may be performed by the wayside or in the forest, merely by laying on the ground an areca nut and a betel leaf with a word of prayer to some divinity.

The priesthood is not closely organized, but there are recognized several classes of official functionaries among whom ceremonial activities are distributed with a fair degree of distinctness. The chieftain, who is both the civil and ecclesiastical head of his village, repeats the central liturgies of the great festivals and offers the sacrifice; a body of warriors known as *magani* have their special functions; priest-doctors, who have some knowledge of magic as well as of the art of healing, assist in times of sickness, at harvest ceremonies, or act as mediums through whom the spirits converse with the people.

The gods of the Bagobo may be grouped in two classes. (1) Gods of exalted rank who live in the nine heavens above. They are felt to be remote from human affairs and neither help nor harm is expected from them, hence no devotions are addressed toward them. These spirits occupy an important place in the mythical songs and romances which the people delight to tell, but the interest is purely of a literary sort, and it is probable that these divinities are of foreign origin. (2) In intimate relation to the daily life are many unseen beings who have charge of the physical world; who act as divine protectors and helpers of man; who direct industries; who stimulate men to fight; and who, in their several departments, receive the prayers and gifts of the people.

Yet, less concerned is the Bagobo with gods than with demons, so far as the routine of daily life is involved. Countless pains and miseries come to him through the direct manipulation of fiends called *buso*, who, in all events, must be propitiated with offerings, tricked by subterfuges, banished by magical rites. These evil beings, some anthropomorphic,

some zoomorphic, dominate the Bagobo's attitude toward life and death, and keep him constantly on the watch lest he be out-manoeuvred, and thus become a prey to bodily suffering. Disease may also be caused by magical means, or because of the transgression of some custom or taboo, and to forestall such evil the behavior of the Bagobo is checked or redirected by rigid prohibitions at many points.

Firm in his conviction that he must look to the supernatural for the source of bodily pain, he proceeds, consistently, to wrestle with a throng of diseases just as he would strive against any other outbreak of hostile demons. The methods recognized as efficacious are of three sorts, any one of which may be used either by itself or in combination with the other two. (a) By an act of devotion; (b) by magic; (c) by native *materia medica*. The author gives in considerable detail, the formal ceremonials related to the curing of disease and the bringing of prosperity to the tribe, as well as those connected with harvesting, hunting, marriage, death, and the like.

Closely related to this belief in demons is that of life after death, for each individual has two souls, the one of the left side which is evil and which becomes a *buso* or demon, and the one of the right side which goes to the one Great Country beneath the earth where it lies forever. At the entrance to the Great Country is the Black River in which the spirit bathes his joints and thus becomes naturalized to the world of spirits. In his eternal home he continues his life as on earth during the hours of darkness, but at the rising of the sun all is changed. Each spirit plucks a broad leaf, twists it into a vessel and seats himself on it, and there sits, waiting, until the hot rays of the sun cause him to dissolve, leaving the vessel full of water. When night returns, he resumes his personality and takes up his work or dance as if no break had occurred.

Human sacrifice, which plays such an important part in the religious life of this people, is described, as is also the peculiar body of brave men known as *magani*, who receive their name and distinctive garments by killing a certain number of their foes. This leads to a full discussion of the sacrificial rites still practised by the pagan tribes of Mindanao, and to those formerly carried on by the Christianized Filipino.

The final chapter deals with the problem of the sources of Bagobo ceremonial and myth. The author finds several points in which these seem unique, yet there still remains the fact of the existence of a mass of ceremonial rites and magical usages common alike to the Filipino and Bagobo, and which may point to a common origin. The similarity of many of the customs to those found in other parts of Malaysia and the unmistakable Hindu tinge to the mythology are likewise noted.

The whole discussion of this complex problem is carried on with admirable caution and may well serve as an example to investigators who are inclined to establish relationships and migrations on data much less conclusive than are here presented.

The volume is of first importance to all students of Malaysia, but is also one to be recommended to all who are interested in primitive religion and folklore.

FAY-COOPER COLE

Population of the Philippine Islands in 1916. H. OTLEY BEYER. Philippine Education Co., Manila, 1917.

This paper, which appears in an English-Spanish edition, seeks to give an accurate estimate of the population of the Philippines up to January 1, 1916. The last comprehensive effort of this kind was the Census of 1903, taken under the authority of the Philippine Commission and with the aid of a large body of assistants. The present study is based on the author's wide knowledge of the Philippine peoples, supplemented by information gained through a series of questions sent out to provincial and municipal authorities, school teachers and others favorably situated to obtain accurate data; while the sketches of the various ethnographic groups are drawn from a careful survey of practically all known literature. The result is the most important general paper so far published on the inhabitants of the Philippines.

A list of the recognized ethnographic divisions is followed by groupings according to religious beliefs, economic and social progress, language and dialects. Next comes a careful estimate of the population by islands, provinces, and electoral districts.

Part II is by far the most valuable portion of the work, from the standpoint of the anthropologist. In this the author gives a brief sketch of the physical types, the language, and the distinguishing elements in the culture of each of the ethnographic groups. He has sifted the anthropological literature carefully and his descriptions of the social and economic life can be questioned only in a few minor details; however, the reviewer must express skepticism concerning several of the physical types which Professor Beyer believes he has discovered in the population. According to this paper he finds that traces of the following types can still be distinguished in the Islands: Malay, Indonesian, a short aboriginal Mongoloid, a tall Mongoloid, Negrito, Papuan or Melanesian, Australoid, and Ainu. These terms are not defined, or any proof offered; however, he promises to present this data in a forthcoming paper entitled "Ethnographic Grouping in the Philippines."

The volume concludes with an excellent bibliography of the more important titles of an ethnographical nature.

The book gives evidence of much careful labor and will serve as a valuable handbook to all who are interested in our island possessions.

FAY-COOPER COLE

A Grammar of Lepanto Igorot as it is Spoken at Bauco. MORICE VANOVERBERGH. (Bureau of Science, Division of Ethnology Publications, vol. 5, part 6, pp. 329-425. Manila, 1917.)

It is gratifying that after a lapse of four full years the Division of Ethnology Publications of the Bureau of Science of the Philippine Islands are once more in progress. Students of the native races of the archipelago will certainly be unanimous in hoping that this valuable series, once so active in its progress, will hereafter continue its accumulating career unchecked.

The present monograph is a missionary's grammar of the Igorot dialect spoken at Bauco in eastern Lepanto. It is intermediate between the Kankanay of Bugias in Benguet to the south and the Bontoc to the north. It agrees with the former in preserving *d* and *b* where Bontoc has *ch* and *f*, but like Bontoc has *e* (*ö*) for the *pepet* vowel where Kankanay has *u*. The author's modest preface should disarm criticism even if his work were of less merit. Such strictures as follow are made only because it seems that a somewhat different presentation would have definitely increased the utility of an undertaking which in general can only be commended.

Like most pioneers, Mr. Vanoverbergh makes his treatment more formal than the language demands. This is evident in his beginning the consideration of the noun with "gender," of the adjective with "comparison," of the verb with "the auxiliaries to have and to be"—none of which exist in the language. It is no more difficult to describe a language according to its own peculiar traits than according to the traits of the languages of Europe. It is in fact easier, as well as a more serviceable proceeding. Authors situated as Mr. Vanoverbergh is, are usually actuated by practical rather than philological considerations, and often out of reach of literature collateral to their investigations, but there is scarcely one of the many operating in Oceania whose work would not be strengthened by a grounding in the classic "Short Comparative Grammar" of Codrington's *Melanesian Languages*.

However, as soon as the content of the present work is examined without reference to the form of presentation, its solid value becomes

apparent. All important processes of the language seem to be treated; and the prefixes, infixes, and suffixes with which its business is carried on are given with reasonable or perhaps even exhaustive completeness. The elements that are so important in idiom, and so often neglected—adverbs, conjunctions, interjections—are presented in gratifying detail. The orthography is fixed by reference to the *Anthropos* alphabet and accent is marked throughout. The definitions of the use and meaning of grammatical elements are generally precise, the examples well chosen from what is obviously a wide range of knowledge. The arrangement and explanation of the passive is not as clear as it might have been; but the distinction of intransitive verbs into those respectively active and passive in English, evidences a more penetrating handling. A very few pages of continuous text with word for word translation would have greatly enhanced the utility of the work for the practical student as well as the philologist, but this desideratum seems only rarely to impress itself upon any but the investigator taught by experience in comparative work.

This grammar is one to be grateful for, and the dictionary and collection of songs that are promised will be awaited with anticipation.

A. L. KROEBER

SOME NEW PUBLICATIONS

De Aranzadi, T., and De Hoyos Sainz, L. *Etnografia, sus bases, sus metodos y aplicaciones a España*. 239 pp. Madrid, 1917.

Gifford, Edward Winslow. *Clans and Moieties in Southern California*. (University of California Publications in American Archaeology and Ethnology, vol. 14, no. 2, pp. 159-219, 1 fig.) Berkeley, March 29, 1918. Price 75 cents.

Laufer, Berthold. *Religious and Artistic Thought in Ancient China*. (Art and Archaeology, vol. VI, no. 6, pp. 295-310.) 1917.

Müller, Max, and Scott, James George. *The Mythology of All Races, Vol. XII: Egyptian and Indo-Chinese*, pp. xiv, 450, 232 figs., XXI pls.

Pope, Saxton T. *Yahi Archery*. (University of California Publications in American Archaeology and Ethnology, vol. 13, no. 3, pp. 103-152, pls. 21-37.) Berkeley, March 6, 1918. Price 75 cents.

Sapir, Edward. *Yana Terms of Relationship*. (University of California Publications in American Archaeology and Ethnology, vol. 13, no. 4, pp. 153-173.) Berkeley, March 12, 1918. Price 25 cents.

Schwerz, Franz. *Die Völkerschaften der Schweiz*. (Studien und Forschungen zur Menschen- und Völkerkunde, vol. XIII, pp. vii, 307, tables and charts.) Strecker und Schroeder: Stuttgart, 1915.

Waterman, T. T. *The Yana Indians*. (University of California Publications in American Archaeology and Ethnology, vol. 13, no. 2, pp. 35-102, plates 1-20.) Berkeley, February 27, 1918. Price 75 cents.

Wilson, Gilbert L. *Agriculture of the Hidatsa Indians. An Indian Interpretation*. (University of Minnesota Studies in the Social Sciences, No. 9.) Minneapolis, November, 1917. Price 75 cents.

DISCUSSION AND CORRESPONDENCE

"THE MATRILINEATE AGAIN"

UNDER this title Dr. A. L. Kroeber contributes to the October-December number of the *American Anthropologist* a reply to my article on "Matrilineal Kinship and the Question of its Priority" in the fourth volume of the *Memoirs of the American Anthropological Association*. His reply is a powerful statement of the case for the priority of patrilineal institutions in North America. It is based upon the claim that the matrilineal peoples were in most cases the most advanced in civilization, and that,

the evidence as a whole indicates the very powerful probability that nearly every nation in North America has reached the matrilineal condition from the patrilineal, the unilateral reckoning from the bilateral, exogamy from a previous non-exogamic state; or at least that the apparent drift, so far as our mental eyes can at present follow it, has been predominantly in this direction.

He does not discuss the specific evidence for the former prevalence of matrilineal institutions in tribes now patrilineal. On the contrary, he reproaches me for so doing with "special pleading." He goes on to say:

Every possible contrary instance is examined, harried by cross examination, confronted by contradictory witnesses, its credibility or inherent probability doubted on special or general grounds.

This is an astounding charge. I have always understood that the examination of the disputed facts was the best way—in many cases the only way—to arrive at the truth. And this is precisely what I have tried to do—not as Dr. Kroeber suggests, as an "attorney seeking judgment in favor of a client," but as a dispassionate scientific enquirer. As I stated at the opening of my article, I proposed

to enquire what are the cases in which descent (and therefore kinship, for kinship depends upon descent) is counted through the father, and in which this mode of reckoning has been claimed as so far primitive that it is impossible to get behind it to any earlier definite social order. Having discovered them I shall endeavor to test the claim by the help of such means as are at my disposal. These cases are chiefly found in North America and Australia; accordingly it is to them that our attention will be directed.

My article was not devoted exclusively to North America. But it would

have required a volume to discuss adequately the facts throughout the entire world, and it was unnecessary to do so where they were not in dispute. I therefore selected only the two areas in which the facts were contested, and confined my attention to the cases disputed. I submit that this procedure was perfectly regular—in fact was exactly the procedure demanded by the present state of the scientific controversy.

This rejoinder must of necessity be short. I need not here discuss the details of the five propositions laid down by Dr. Kroeber. Let it be granted for my present purpose that

Within the confines of exogamy, patrilineal reckoning prevails among the less advanced nations [of North America], and every important acme of civilization is situated among matrilineal peoples.

The geographical relations between the matrilineal and patrilineal peoples, pointed out by Dr. Kroeber himself, strongly suggest that the relation between their institutions is something closer than one founded on an independent evolution from a common origin in a bilateral reckoning, such as rules among the Eskimo—to say nothing of the fact to which I drew attention, that no people having once attained to a bilateral reckoning had ever been known to abandon it in favor of reckoning along one line solely. Neither Dr. Kroeber nor anyone else has, so far as I am aware, ever attempted to produce evidence of such abandonment; and though he alleges it as probable, the allegation is, I venture to think, a mere guess for which no solid ground appears.

This being so, either matrilineal reckoning must have emerged out of patrilineal reckoning, or *vice versa*. Now the greater general advance of patrilineal peoples elsewhere than in North America is admitted, as I understand, by Dr. Kroeber and those whose views he shares. But it is claimed that in North America the case is different, and that, since there the peoples possessed of the higher degree of civilization are found to be generally, though not universally, matrilineal, therefore the patrilineal organization is the more primitive. This inference is, however, by no means inevitable. The greater advance in civilization of patrilineal peoples in other parts of the world points to an organic connection between paternal descent and progress in civilizations, which should induce a close scrutiny of the relation between maternal descent and the other constituents of the civilization of the Iroquois and similarly organized tribes, with a view to defining exactly that relation and ascertaining its history. Dr. Kroeber complains that I do not attempt to correlate the social institutions of peoples with other aspects of their civilizations, or with these civilizations viewed as such.

I am quite alive to the importance of doing so, but that was not my business in the offending article. There I was occupied in pointing out certain facts in the institutions of Sioux and Algonkins which had dropped from sight amid other considerations. There is one thing that cannot be left out of account in such correlation: the connection which seems to exist between matrilineal institutions and an agricultural civilization, based as it is upon female organization and labor, not only in North America, but elsewhere. This would lead to resistance of paternal, and a consequent prolongation in strength of maternal, institutions. On the other hand, the nomadic life of hunters would result in the enhanced importance to the family and authority of the husband and father, and tend to change the reckoning of descent, as it has done in Australia. If so, then matrilineal reckoning cannot be relied on as an essential constituent or a product of the advanced civilizations of North America, for it may be a survival of an earlier stage, and conversely patrilineal institutions may be a relatively modern development. In these circumstances it is important to enquire whether the institutions of partilineal tribes contain any evidence of a previous matrilineal reckoning; for this would be crucial. Accordingly this is what I have done, and I submit with success. For such evidence has been found, embedded like fossil remains in geological strata, among the very tribes where it was summarily denied. The evidence may be challenged, or its effect may be attenuated, or conceivably even destroyed by explanation; but it cannot be merely ignored or pooh-poohed as "special pleading." I will not retort the charge of special pleading; but I cannot think that Dr. Kroeber's description (*American Anthropologist*, vol. 19, p. 578) of my procedure is generous, or even quite fair. So long as the evidence stands it must be correlated with the other facts; it will not do simply to appeal to general considerations.

But while general considerations have no validity in the teeth of actual facts, they may legitimately be arrayed in support of such facts once proved. Dr. Kroeber thinks otherwise. The influence of bloody wars, with their results in the destruction, amalgamation, or banishment and wandering of tribes, and the consequences, direct and indirect, of white settlement cannot be denied; but they are waived aside as irrelevant. Nor does he attempt to account for the extraordinary *colluvies gentium* in California. The cases that brought together tribes so alien from one another in blood, language, and institutions must have operated from one side of the continent to the other, on tribes in various stages of evolution, through a long period of time, and must have dislocated and

disrupted many ancient traditions and institutions. "Integrate the evidence," by all means. That is a scientific course; but before you "accept the results that eventuate" take care that you have not overlooked important though possibly awkward, facts, and that you have given due weight to every argument.

With regard to any purely incidental allusion to the case of the Sia, as Dr. Kroeber puts the matter it would seem as if I had invented the explanation of their violation of their laws of exogamy, whereas I simply quoted Mrs. Stevenson's account. Does Dr. Kroeber deny it? I did not refer to the case of the Zuñi because their organization was not in dispute—not because, as he sarcastically suggests, "the case is too lacking in significance to refute or mention." It is significant; but I am sure that its significance is exactly such as would be convenient to the advocates of the priority of patrilineal institutions.

Finally, I am happy to concur with Dr. Kroeber in his disclaimer of nationalistic aims. The field of science and scientific history is, as he says, international. Science knows no national boundaries. Least of all could I, who am so largely indebted to the anthropologists of the United States, be guilty of any nationalistic aims; and I am equally sure that Dr. Kroeber on his side needs not to plead for acquittal. What both of us desire as the result of this friendly encounter is the advancement of scientific truth, and nothing else.

E. SIDNEY HARTLAND

HIGHGARTH,
GLOUCESTER, ENGLAND

COMMENTS ON THE ABOVE

I GRATEFULLY acknowledge Mr. Hartland's sustained courtesy under provocation. We appear to differ chiefly in the degree of objectivity which we accord to isolated "facts" when they collide with correlations. Biologists control a mass of integrated data which show that in general insects begin their careers as eggs and pass through a more or less larval and often pupate form before they reach the final or adult stage. If a newly discovered butterfly is known only in the adult form, we do not therefore conclude that the species lacks the preceding forms. In fact, should it be asserted that any given species had been observed to change from imago to larva, the observation would be doubted, not accepted as a "fact," on the ground of being contrary to all known cognate data. The observation would have to be repeated on the full life history of the species in question, and under every safeguard against error, before serious attention could be granted it.

Now, what corresponds to this complete life history of an insect, in the issue between Mr. Hartland and myself, is the history of an individual tribe over a long enough period for it to undergo a pretty profound modification of its institutions. Such a time would normally be a much longer period than that during which we have even known any American tribe to exist; to which it may be added that all we ordinarily have of such histories is a paragraph by an explorer of a century or two ago, some subsequent notices by travelers or soldiers of a literary bent, and perhaps a more or less intensive study by one or more subsequent ethnologists. All this is not Mr. Hartland's fault; but it leaves his case much in the status of that of the insect observer who saw a butterfly in the box in the evening, a caterpillar in its place in the morning—and then his wife came along and threw away the box without any record having been made of its chinks—nor in fact whether the lid was not left open over night. The kind of intimate, reliable, institutional history of the individual North American tribes that Mr. Hartland could properly base his case on, simply does not exist. It probably never will be recovered in the necessary fulness. We must therefore fall back on inferential probabilities based on averaged experience. This I perhaps insist on doing to an exaggerated degree; but he seems to me to do to an insufficient degree, so far as the North American matrilineate is concerned. If anyone reported that the normal stature of men in Pike county, Missouri, was seven feet, I should not hesitate to rule such a "fact" out of the realm of belief, even though six observers alleged it and the occurrence does not transcend known physiological possibility. I should not even trouble to sift the reported evidence in detail, much less make a trip to Missouri with a measuring rod. I do not wish to suggest that Mr. Hartland's attitude is as gross as this comparison might imply. I do not even charge him with unreasonable naïveté. But I must ask to be excused from discussing evidence as fragmentary and miscellaneous as that on which he relies, so long as a putting together of all the really linkable evidence points to opposite conclusions.

Mr. Hartland brings forward without pressing very far one argument that I admit to be strong. I have appealed over the heads of isolated bits of evidence to the findings of the North American evidence as a whole. He in turn is correct in appealing from this to the evidence of the whole human history. But of course no mere count of souls, nationalities, or periods suffices. Just as mammals are more numerous, more developed, and more generally important than the tunicates, but the latter or the still smaller group containing the amphioxus are of

equal significance for an understanding of the course of chordate evolution, so native American culture must be equated with almost the whole of Old World culture. Each is a coordinate unit, essentially self-developing, but necessarily to be taken as a unit because all its parts or members have for a long time past stood in more or less close connection with other parts thereof. The only other such unit of whose distinctness we are at all sure is Australia. Even therefore if both Eurasia-Africa-Oceania and Australia proved demonstrably to have changed from matrilinear to patrilinear descent, the case would stand only two to one against aboriginal America; which is not sufficiently overwhelming to compel a reconsideration of the latter field. Such a result might indicate that there existed a preponderant inclination for "paternal descent and progress in civilization" to be associated; it would not establish the "organic connection" between them of which Mr. Hartland speaks, and which appears to be the emotionally colored origin of his attitude, just as it is the irritating stimulus against which I inevitably react.

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"MORE LIGHT:" A REJOINDER

A SECOND reading of Professor Dixon's remarks (*American Anthropologist*, vol. 20, p. 124) leads me to the conclusion that we are in almost complete, though latent, agreement. Professor Dixon gathers from my two reviews of his book (1) that I reject the influence of migration and diffusion on the development of Polynesian culture; (2) that I regard the evolutionary type of cosmogony as older than the creative, wrongfully imputing to Professor Dixon the same view; (3) that "explicitly for New Zealand, and implicitly for the whole of Polynesia" I dogmatically assume a homogeneous population. It will be relatively easy to dispel these misunderstandings.

1. I emphatically believe that the principles of culture contact and migration are as applicable in the Polynesian as in every other field of ethnology. But it is not necessary to apply these principles in each and every case.

2. I regard the *creative* as older than the evolutionary type of cosmogony. In a sentence of the *New Republic* review the two adjectives are interchanged. I am heartily sorry for this error, but precisely because it is so glaring, very few readers, I hope, were misled as to my intended meaning, which I think is manifest from the context.

3. My statements are quite dissociated from any belief in either the homogeneity or heterogeneity of the Polynesians.

What, then, is the cause of the whole difficulty? There is, to be sure, one difference in principle but not of the type suggested by Professor Dixon's assault. Professor Dixon's mode of approach is an exclusively ethnographical one, mine is both ethnographical and ethnological. Mythology must be studied topically no less than from the viewpoint of diffusion. We know that in the same tribe different versions of the same tale occur which cannot be derived from alien sources. We know specifically that precisely such esoteric transformations and complications as I suggest for Maori mythology have taken place elsewhere. Methodologically it is therefore proper to apply the same efficient cause to the interpretation of a like differentiation in New Zealand. This procedure seems methodologically superior to that of assuming a migration or alien influence for variations. Otherwise we should have to evolve an indefinite number of hypothetical migrations to account for the simplest of ethnological phenomena. If, for example, a Plains Indian tribe employs both triangular and diamond-shaped designs, it is not contrary to the laws of thought to assume that the diamond may have arisen within the group by a juxtaposition of triangles instead of having been imported from elsewhere.

It seems desirable to add one general remark. Readers of Professor Dixon's discussion might infer that my notices of his book were depreciatory in tone; the contrary, however, is the fact.

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DR. FRACHTENBERG'S "BACILLUS REDUCTIONIS"

As a retort to Dr. Frachtenberg's impetuous onslaught¹ against my recently published "Parallel . . ." I should like to advise him to read the accompanying two papers, which he has evidently overlooked, entitled "Iroquoian Clans and Phratries"² and "Growth and Federation in the Tsimshian Phratries."³ It was first intended to publish these three papers together, the brief concluding "Parallel . . ." being merely a corollary of the more substantial papers that preceded.

When Dr. Frachtenberg has read these articles it will presumably dawn upon him that—notwithstanding his cocksureness as to the value of his own method—he has quite misunderstood and misrepresented

¹ *American Anthropologist*, vol. 19, p. 581.

² *American Anthropologist*, vol. 19, p. 392.

³ *Proceedings of the Nineteenth International Congress of Americanists*, Washington, 1915, p. 402.

the facts. In the first two papers the Iroquoian and Tsimshian social units were described with a view to eliciting their respective differences; and the "Parallel . . ." was a brief summary of such dissimilarities.¹

What seems to have led Dr. Frachtenberg astray is a queer misconception of the use of the word *parallel*. In English as well as in the Romance languages, the word *parallel* does not exclusively mean *similarity* but also *comparison made* (παράλληλος, side by side). For instance, Carlyle says,

He runs a laboured *parallel* between Schiller, Gæthe and Kotzebue; one is more this, one is more that.²

In my "Parallel between the Northwest Coast and Iroquoian Clans and Phratries" it is quite clear from the contents that I had a similar meaning in mind.

That the contrast between both systems, in "Parallel . . .," was convincing may be gathered from Dr. Frachtenberg's candid, if not intentional, admission. In this connection, it should not be forgotten that Morgan, Fraser, Lang, and others have upheld the theory that phratries and clans in the various parts of the world are not only comparable in their contents but also in their growth. Dr. Goldenweiser has lately accepted the general hypothesis bearing on the growth of phratries and their historical association with clans. On a few occasions, particularly at the annual meeting of the American Anthropological Association in 1913, Dr. Goldenweiser and I had a debate in which we disagreed on that very point with reference to the Iroquoian peoples in particular. My above-mentioned papers, in fact, were the outcome of that controversy.

Now one may wonder how, without first-hand information in that field, Dr. Frachtenberg has come to reject the theory of Morgan, Lang, and Goldenweiser? Would it not be my very "Parallel . . .," misconstrued though it is in its purpose, that has made a convert of him? Be that as it may, Dr. Frachtenberg has not injected any new element into the controversy and, curiously enough, he upholds my view while apparently combating it.

The last paragraph in "Parallel . . ."—which adds fuel to Dr. Frachtenberg's ire—is nothing but a veiled invitation for an adverse

¹ Dr. Frachtenberg should not be swayed into accepting this view to the extent of overlooking some interesting similarities evinced in the growth of phratries and clans in the two areas; the gradual splitting up of growing units into integrant parts and the federation of unrelated groups are in both places to be found in operation.

² *Imperial Dictionary*, vol. III, p. 371.

opinion as well as an acknowledgment of incompetence regarding the history of many moiety societies of the Plains or of Southern British Columbia, and of recently discovered (by Mr. Gifford) phratries in California. Are all those social units historically disconnected or not, in their foundation no less than in their present form? Here lies a problem that might some time be usefully attacked.

We would not uncharitably suppose that, in his challenge, Dr. Frachtenberg was simply attempting to stave in an open door. Something more worthy must have absorbed his attention and stirred up his marked aptitude for bristling irony—that is, ornamented with exclamation points. The following utterance¹ is no doubt a symptom of what he had at the back of his mind:

Unfortunately, he (Barbeau) seems to have become inoculated with the *Bacillus Reductionis* like some other of his anthropological co-workers and is trying to arrive at sweeping conclusions which are based upon inadequate and improperly digested data. . . .

As my co-workers and I have no knowledge of medicine, we all feel puzzled and worried over the Doctor's warning as to that dreadful inoculation coming on top of digestive troubles.

The only hope left to us is that the learned gentleman may have erred in his diagnosis and that the *Bacillus reductionis* may after all have been cultivated in his own laboratory.

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¹ *American Anthropologist*, vol. 19, p. 581.

PROCEEDINGS OF THE ANTHROPOLOGICAL SOCIETY OF WASHINGTON

Meeting of October 2, 1917

THE 513th meeting of the Society was held at the United States National Museum, October 2, 1917, at 4:30 p.m. At this meeting Dr. Aleš Hrdlička, curator of physical anthropology, U. S. National Museum, addressed the Society on "Bohemia and the Bohemians," illustrating his address with lantern slides.

Dr. Hrdlička said:

Bohemia is not a large country but one with a great history; and while among the oldest in Europe and one of the most battered by fate it is struggling vigorously to regain its freedom, which it lost in the dark period of the seventeenth century. Its people have been endowed with an unquenchable love of liberty and its free sons are now fighting in every allied army.

The speaker then noted the geographic position of Bohemia in the center of Europe, surrounded by a natural boundary of hills and mountains. Its area is about one-fourth greater than that of Switzerland, with a density of population nearly twice as great as that of France, and one-seventh greater than that of Germany. Ethnically the Bohemians are Slavs. The names Bohemia and Bavaria are both of Roman origin, derived from the name of the Keltic tribe of Boii, the forefathers of the Bavarians who may have extended over, or claimed a part of, Bohemian territory at one time. The name Czech (applied to the Bohemians) is, according to old tradition, derived from that of a leader or chief of the people.

Archaeological excavations have shown that the Slavs were in Bohemia long before the beginning of the Christian era. The earliest historical mentions of them occur in the second and third centuries. They were never subject to Rome, and the Germans were their eternal enemies. At the beginning of the seventh century they were a strong political unit and in 630 were powerful enough to severely defeat the Germans. Then began historically the marvelous life-and-death struggle of the Czech people with the German flood that would engulf them, a struggle of thirteen centuries and which has lasted until the present day.

The rich Bohemian literature and archives were repeatedly destroyed by the enemy but enough has been saved to show that those early times

were both idyllic and magnificently barbaric. The people were agriculturists and soldiers. Their organization was patriarchal, their government constitutional, almost republican. The religion of Bohemia was naturalistic and poetic. The priests worshiped under great oaks. There was a supreme deity, and a series of *bělobozi*, or good gods, *černobozi* and *děsi* or demons, *vily* (fairies), *vodníci* (water-spirits, etc.). The burials were by cremation.

From the eighth to the fourteenth centuries the Bohemians were ruled by kings of a strong native dynasty. In 1526 the last of the Bohemian kings perished in a battle with the Turks, and soon afterward Bohemia as well as Hungary joined Austria for mutual protection against the dread peril. This was the beginning of Bohemia's misfortunes. During the 30 years' war the life of Bohemia was nearly extinguished. Not until the nineteenth century came the time of a revival and restitution. Today the nation stands at the head of all those comprised in the mediaeval conglomerate of Austria-Hungary in education, industry and in practically every other respect. It is still shackled and persecuted by Austria but hopes and works for an early victory of the allied arms and with this its liberation. In 1918 Bohemia will have with the allies two small armies of its own, one in France and one in Russia.

Meeting of October 16, 1917

THE 514th meeting of the Society was held in the United States National Museum, October 16, 1917, at 4:30 p.m. Dr. Mitchell Carroll, secretary of the Archaeological Institute of America, delivered a lecture on "The Story of Greece." The lecture was richly illustrated with lantern slides portraying the principal centers of Greek life, such as Olympia, Delphi, Sparta, and Athens, with the monuments of architecture and sculpture that have been most influential in the development of Art.

Dr. Carroll, in introducing his subject, noted our indebtedness to Greece as five-fold, comprising (1) Democracy, (2) Obedience to Reason, (3) Love of Beauty, (4) Letters, and (5) Art. The history of Greece was outlined in seven divisions: (1) The Prehistoric and Heroic Ages to the Dorian Migration, 2000-1000 B.C. (2) The Greek Middle Ages, 1000-500 B.C. (3) From the Persian Wars to Alexander the Great, 500-386 B.C. (4) From Alexander the Great to the Roman Conquest, 336-146 B.C. (5) The Roman, Byzantine, and Latin Supremacies, 146 B.C. to 1453 A.D. (6) The Ottoman Supremacy, 1453-1832. (7) The Modern Greek Kindgom, 1832.

The racial life of Greece was emphasized by Dr. Carroll, who said:

The central fact of all Greek history, from prehistoric times to the present, is the unbroken life of the Greek race. This racial unity rests on common blood, common language, and common institutions.

1. *Race*.—From 650 to 850 the Slavs in Greece outnumbered the Greeks, but the Greeks, being superior in civilization, gradually absorbed them. The process of Hellenizing the Slavonians went on steadily until in about 200 years it was practically complete. Thus, between 850 and 1050 was formed the basis of the modern Greek nation. It contains a large infusion of Slavonic blood, but the strain of Hellenic blood has been perpetual and this has determined the type of the modern nationality.

2. *Language*.—Greek, though for many centuries crude and ungrammatical, never lost its vitality. In organic matters of structure and syntax Greek has never made a compromise with any foreign language. Briefly, its story has been this. About 300 A.D. the spoken Greek language began to diverge from the literary language, but until 750 Old Greek was generally understood by the people. Then came the breach of Greek tradition, due to the Slavs, and by 900 A.D. classical Greek had probably ceased to be generally understood. Between 1100 and 1200 popular Greek began to have a literature of its own, the popular Greek of the thirteenth century differing little from the popular Greek of today. The chief difference between Old and Modern Greek is that one is synthetic and the other analytic.

3. *Character*.—National characteristics of ancient and modern Greeks are; (1) aptitude for city life, (2) ability in commerce, (3) love of mental culture, (4) cleverness. The real core of the Greek nation throughout its history is the agricultural population of Greece proper. The Greek nationality, like the Jewish, has never been crushed out nor lost.

Meeting of November 6, 1917

THE 515th meeting of the Society was held at the U. S. National Museum, November 6, 1917, at 4:30 p.m. At this meeting Prof. James H. Gore presented a paper on "Belgium and the Belgians," illustrated by stereopticon slides.

Starting with the revolution which resulted in the withdrawal of the part of Holland that afterward became an independent kingdom with the name "Belgium," the speaker explained the duality of languages in Belgium and the ethnic differences between the users of the two tongues.

Immediately prior to the present European war one-tenth of the entire population of Belgium were housed in dwellings which, on easy terms, had become or were becoming the property of the occupants.

Thirty-five per cent. of the people had accounts in the savings banks and forty-nine per cent. of the inhabitants, male and female, worked at regular callings. Statistics were given to show the thrift of the people, the fertility of the soil, the extent of their foreign trade, and the variety and magnitude of their industries. On each square mile there were 598 inhabitants, and for each inhabitant the railroads annually carried merchandise having a value of \$145 as compared with Germany's per capita of \$60, and \$30 for the United States.

Considerable attention was given to the agricultural commission—a sort of university extension—which brings to the farmers of the country speakers who tell of recent discoveries and improvements in agriculture that would be of value to the people of each community. To this wise provision can be ascribed a large part of the productivity of Belgium.

The profit-sharing dock laborers of Antwerp were described and it was shown that the prosperity of that port was due to the efficiency of its charging and discharging instrumentalities.

The unique town of Gheel was fully described. In Gheel practically every family cares for one or two feeble-minded persons under the supervision of government officials. If the family is unable to meet the expense of this care it is borne by the state.

Meeting of November 20, 1917

THE 516th meeting of the Society was held at the U. S. National Museum on Tuesday, November 20, 1917, at 4:30 p.m. The speaker was Mr. George Julian Zolnay, who addressed the society on "Roumania and Her People," illustrating his subject by stereopticon slides and by native music rendered by violin and piano.

Mr. Zolnay stated that, with the exception of the Roumanian Jews, there are few natives of Roumania in the United States at the present time, and of these a large majority are from Transylvania and the Bukovina. This accounts for the dearth of accurate knowledge concerning this picturesque country, wedged in between the Carpathian mountains and the Black Sea.

The history of Roumania began in 106 when Trajan conquered Dacia, a country comprising the territory now known as Roumania. At the fall of the Roman Empire in the fifth century the descendants of the Roman soldiers and the Dacian women had become a distinct nationality, speaking a slightly modified Latin which has remained the language of the Roumanian people to the present day. The established religion has remained that of the orthodox Greek Church, although Roumania

was a vassal state of Turkey for more than three hundred years. During the Russo-Turkish war Roumania regained her independence and was proclaimed a kingdom in 1881, later taking her place as a leading country among the Balkan States.

One of the most remarkable traits of the Roumanian is his love of his national music. This music is so distinct from that of all other nations that only the gypsy, who is the professional musician of the country, can render it with the mysterious quality that stirs the Roumanian soul.

Although the misfortunes of war have prostrated Roumania it is to be hoped, in the light of her past history, that she will yet emerge intact to perpetuate her Latin civilization in the midst of her alien neighbors.

Meeting of December 4, 1917

THE 517th meeting of the Society was held in the Auditorium of the U. S. National Museum on Tuesday, December 4, 1917, at 4:30 p.m. At this meeting Dr. Amandus Johnson, of the University of Pennsylvania, addressed the Society on "The Scandinavian Peoples," illustrating his address with lantern slides.

Dr. Johnson said:

The Scandinavian Peninsula has undoubtedly been inhabited by its present occupants for 10,000 years or more. When the climate of the country became tolerable, after the vast icefields receded, tribes of the Aryan race found their way into southern Sweden, and established there the original home of the Germanic peoples. About the year 3000 B.C., at the end of the stone age, considerable advancement in culture had been made, and during the bronze age the decorative instinct of the people found expression in works of art unsurpassed elsewhere in Europe at that period. Later the Hallstatt and La Tène civilizations made their influence felt and finally, about the beginning of the Christian Era, Roman culture became the predominant foreign influence. An extensive trade developed with the western world during the following centuries, and many remains of this intercourse are found in Sweden and Denmark.

The most important period historically is the so-called Viking Age, 800-1000 A.D. Wonderful progress had been made in shipping and navigation. Fleets of the Viking ships appeared on almost every shore. The bold sailors sacked cities on the Mediterranean and Black Seas, ruled Ireland for generations, and conquered parts of France, England, and Spain; they founded Russia, and settled colonies in America and numerous other places. Finally Christianity was introduced and the Scandinavians settled down to a life of peaceful toil. The mental and spiritual reaction following the Viking expeditions was intense. A prose literature grew up, especially in Iceland. This was the most remarkable in Europe at the time and was the only original prose of the Germanic race. With it was coupled a poetry no less important. This art died, however, at about the

time when distinct Scandinavian nationalities began to develop, and from the twelfth century onward we find long stretches of time nearly void of mental activity.

From this period Sweden began to lead a more separate life, but Denmark and Norway were gradually drawn closer together until the latter country nearly lost its identity. Denmark was the leading power of the north until the appearance of Gustavus Adolphus. Then Sweden acquired the supremacy. Through the supreme ability of her leaders she changed the course of European history and for more than a century played the rôle of a great power. In modern times Sweden has produced leading scientists, created a rich literature, and developed large industrial establishments.

After 1644 Denmark was weakened from time to time by the curtailment of her territory until in 1864 she was reduced to her present area. In the fields of science, letters and art, however, she can point to brilliant achievements. Norway paid the price of dependency for many generations, and not until her separation from Denmark can we speak of a worthy Norwegian literature. But in the last century the leadership of the drama belongs to her, and in many lines of achievement some of her names rank among the first.

Meeting of December 18, 1917

THE 518th meeting of the Society was held in the Lecture Hall of the Public Library, on Tuesday, December 18, 1917, at 8 p.m. On this occasion Dr. Daniel Folkmar, U. S. Tariff Commission, delivered a lecture on "Japan: People and Policies," illustrated by numerous lantern slides.

Dr. Folkmar opened his address by asking,

Who are the Japanese? Are they as closely related to the Chinese as many Americans think, or are they a very different race, as the Japanese themselves think? The whole attitude of the Japanese toward the Chinese and toward the American people seems to rest on the assumption that they are not Mongolian, strictly speaking, and that they should be treated as our equals.

The Japanese frequently compare their empire with England, the Island Empire which rules a great part of the world from its favored position in the Atlantic, a position similar to that of Japan in the Pacific Ocean. The Japanese are unquestionably a mixed race, like the English and most of the leading nations of the present day. Five distinct ethnic types are to be found among the Japanese. The most important is the Manchu-Korean type, taller than the others and seen chiefly among the upper classes. The second is the well-known Mongolian type, with a broader face. Perhaps the most important element in the present nationality is the Malay strain, whose representatives are small in

stature. The Ainu preceded both Mongolians and Malays, and it now appears that they, in turn, were preceded by a smaller race of pit-dwellers. According to Keane the Japanese bear a physical resemblance to the Mongolians, but linguistically are more closely related to the northern Asiatic Finno-Tataric stock. From this point of view the Japanese are more closely related to the Koreans than to the Chinese, since the Korean language is agglutinative and that of the Chinese is monosyllabic. Numerous authorities were cited on this and similar problems of the Japanese people. Japan received its profound philosophies from India and China. Thus the native religion of Japan is Shintoism, together with Buddhistic beliefs that came from India, and Confucianism from China.

Concerning Japanese policies Dr. Folkmar said:

There is no doubt that an exclusive policy dominated the national policies of Japan until Perry, the American, broke down the barriers. This act is now regarded by the leaders and educated classes as one of the most fortunate events in their national history.

Dr. Folkmar spoke in high encomium of the manner in which the Japanese Empire has kept its word in restricting the emigration of Japanese to the United States, and said:

There can be no doubt of the wisdom of taking the Japanese at their word in the recent convention that has been signed regarding the "open door policy."

Meeting of January 15, 1918

THE 519th meeting of the Society was held in the West Study Room of the Public Library, January 15, 1918, at 8 p.m. The program consisted of a general discussion of War Anthropology, led by Dr. Aleš Hrdlička, curator, division of physical anthropology, U. S. National Museum.

Taking as his subject "War and Race," Dr. Hrdlička first directed attention to the very general and serious apprehension that the present war may have an untoward dysgenic effect on the race, saying that there exists, even among medical men and some men of science, a fear of the effect of shattered constitutions and the lasting results of shocks, strains, exposure and wounds, together with an acquisition of new diseases. The speaker said:

These assumptions are enough to make the pessimist despair of the future of the race, but happily these assumptions are not entirely correct. . . . In the first place we have no scientific basis for the belief that any of the warlike nations of the past have actually degenerated physically as the result of wars. . . . Unquestionably there are losses from every great war, and in these I include the

debilitating effects of wounds and disease, but fortunately these appear to be only temporary.

There are wonderful laws working on living nature, including humanity. One of these is the elimination of the unfit. Another is adaptation, still another is restitution, and finally there are the laws of compensation. These laws have taken care of war-ridden mankind in the past, and as they work with undiminished vigor they can safely be expected, with such intelligent assistance as can now be given, to accomplish still more in the future.

Treating of the action of these laws Dr. Hrdlička noted that many afflictions caused by the war are curable and others are not transmitted to progeny. The most dangerous diseases of previous wars have largely been eliminated by preventive means, while science is already coping with new conditions that have arisen.

The speaker then recounted some of the compensations that will arise from the war, chief among which he placed the impetus given to the struggle against alcoholism. Important also among the compensations will be the great intellectual stimulus, the social and national regeneration, and the raising of this nation from an isolated and somewhat selfish position to that of a world power in the best sense of the term and for the good of humanity.

In the discussion which followed this communication the office of the Surgeon General U. S. A. was represented by Lieut. Sidney Morgan, Sanitary Corps, U. S. N. A., who spoke on the surprisingly large percentage of wounded men who, by expert care, are returned to their homes fitted to be useful members of society. Mr. Frank D. Tansley, ex-president of the Patria Club of New York City, stated that the ratio of casualties in the present war is about the same as that in the Civil War, from which the nation has been able to recover. Mr. E. T. Williams, of the State Department, noted that there may be a deterioration of the race in time of peace, due to industrial conditions and crowding of factories. Dr. John R. Swanton contrasted imperialistic and emulative civilizations, to the advantage of the latter; Mr. James Mooney emphasized the thought that psychology is the dominant factor in race differentiation; and Dr. Leo J. Frachtenberg spoke of predominant elements in every race. Rev. John M. Cooper mentioned an essential vitality which is the outcome of circumstance and which has been, to some extent, lacking in American youth but which may be developed by present conditions.

Meeting of January 29, 1918

THE 520th meeting of the Society was held in the West Study Room of the Public Library, January 29, 1918, at 8 p.m. At this meeting Dr.

Leo J. Frachtenberg made an address "Poland and on the Polish Question."

Meeting of February 12, 1918

THE 521st meeting of the Society was held in the West Study Room of the Public Library, February 12, 1918, at 8 p.m. Dr. Joseph Dunn, of the Catholic University of America, was the speaker of the evening and presented an interesting paper on "Scotland."

The Scotch reached Scotland from Ireland and are not the descendants of Gaelic Celts who had been pushed north by a later (British) invasion of Britain. The first authentic information on Scotland dates from the time of the Romans, 79 A.D. Roman rule in Britain came to an end in 410, and Britain then ceased to be a part of the Roman Empire. The population of Scotland is made up of Pictish, Irish, British, Saxon, Danish, and Norman elements, all of them Indo-Celtic, the three first, Celtic, the three last, Germanic peoples. The Picts contributed the bulk of the population, but were overcome by the Scotti (Irish), who had settled in Dalriada, a part of the present county of Argyle (Airer-Goidel—"Margo Scottorum"). The Scotti then became the dominant people. Brythonic Celts dwelt in Strathclyde; their chief city was Dumbarton (Dun Brettan, "Fort of the Britons"). Toward the close of the eighth century, the Danes appeared and ravaged the coast settlements and the isles. The Saxons first appeared in 428 in Britain. In the 11th century Norman refugees first crossed the border into Scotland.

The first Irish colonization in Scotland took place toward the end of the second century, but the kingdom of Dalriada was not effected until the close of the fifth. It is these Scotti who have given their name to Scotland. The relations between the two countries were very close and lasted for a thousand years, or at least up to the Reformation, and the early literature and civilization of Scotland belong to Ireland. The Scottish Gaelic reached its greatest extent in the eleventh century, when the Anglian-Celtic linguistic line ran from Tweed to Solway and to the Pentland Frith. The line has since been receding. Of the three parts into which Scotland is naturally divided, the larger part of the central and all of the northern, with the exception of the northeast part of Caithness, the Orkneys and the Shetlands, is Gaelic-speaking. The 1911 census showed 202,398 Gaelic speakers in Scotland, of whom 18,400 were monoglots.

According to legend, the name Scotch is derived from *Scota*, a daughter of one of the Pharaohs. The word is probably related etymologically to the German *Schatz*, and means "masters, owners." Originally, and therefore in all medieval Latin texts down to the end of the eleventh century, it meant only Ireland. Since that date it means specifically Scotland. The Scotch Gael never calls himself Scotch, but Gael, or, to indicate his country, *Albanach*. English-speaking Highlanders, even though Scotchmen, are Saxons in the mind of a Gael. In the fifteenth century, when English became the predominant speech in the Lowlands, the English and non-Celtic Scotch called Gaelic "Erse." Since the sixteenth

century the name Scotch has been applied to the English spoken in the Lowlands. So, by a strange freak of fortune, Scotch, originally applied to a variety of Celtic, has come to mean Broad Scotch or Quaint English, a language of Germanic origin.

The distinction made between the Highlands and Lowlands of Scotland is correct merely so far as the physical configuration of the country is concerned, but incorrect if a racial significance is read into it. There is a mistaken notion that Scotland is a country of two races, Celtic in the north and Teutonic in the south, and that the latter element has displaced the former. No doubt the Lowland Scotchman is a person of very composite blood, but he is above all a Celt.

When Scotland was in possession of complete autonomy she enjoyed unrivaled prosperity. She was spoken of on the Continent as "a nation of heroes," and the French proverb "*Fier comme un ecossais*" is still current. Many treaties of alliance were made with France, and Scottish merchants, traders, and scholars were known all over Europe. The disaster at Culloiden (1746) would appear to have crushed Scottish nationality out of existence. The incorporating Union of 1707, "which was carried by force and fraud" (Professor William Smith), reduced Scotland to the humiliating level of an appendage of England. Lord Roseberry called Scotland "the milch cow of the Empire," and the Marquis of Bute and others have estimated that the dead loss to the country as a result of the Union is from twelve to thirteen million pounds per annum. As a result of the "clearances," the crofters and cotters have had to move to the towns and their places have been taken by rich men who have turned the country into "sanctuaries" for deer and grouse. The present-day Scotch republicans, who represent a party which came into existence at the time of the French Revolution, are now taking steps to see to it that the principle of "self-determination" is applied to Scotland.

Meeting of February 26, 1918

THE 522d meeting of the Society was held in the West Study Room of the Public Library, February 26, 1918, at 8 p.m. On this occasion Dr. Peter Alexander Speek of the Library of Congress addressed the Society on "The Problem of Race and Nationality in Russia."

Pointing out the difficulties of a definition of the term "nationality," the lecturer stated that race is a perpendicular division of mankind, a group of people separated according to ethnological and anthropological differences which have resulted mainly from the natural surroundings in prehistoric times, and that nationality is a perpendicular subdivision of a race or races, a group of people with common ways and forms of life, but different from other groups because of historical development under the influence of the different geographical conditions and social forces. Thus nationality may be expressed more or less in everything which is native to a human being and characteristic of his existence—in physical form, in mental and spiritual development, in economics, politics, science, arts, moral principles, customs, and habits.

The speaker described Russia as a conglomerate of a large number of highly varied countries, races and nationalities united by conquests into one body politic, ruled up to the time of the revolution by the same monarch and the same laws and institutions.

In 1914 the population of Russia was nearly 180 millions, the race composition of which was as follows: Indo-European, about 80 per cent.; Ural-Altaic, 14 per cent.; Semitic, 4 per cent.; indefinite, about 2 per cent. The statistics of nationality were as follows: Indo-European race: Great Russian, about 44 per cent.; Little Russian, 18 per cent.; Polish, 6 per cent.; White Russian, 5 per cent.; German, about 2 per cent.; Lithuanian, 1 per cent.; Lettonian, 1 per cent.; Armenian, 1 per cent. Ural-Altaic race: Turkish-Tartar, 11 per cent.; Finnish, 2 per cent.; Estonian, 1 per cent. Semitic race: Jews, 4 per cent.; other minor nationalities of the above races, 2 per cent. of the whole population. The last Russian census shows that there were 123 different and distinct nationalities living in Russia. The Great Russians, about 44 per cent. of the population, ruled all the other subjugated nationalities, *i. e.*, 56 per cent. of the whole population.

The policy of the Russian monarchy was to Russianize the non-Great Russian nationalities by violence. This policy is to be explained, in part, by the teachings of Pan-Slavism. Pan-Germanism and Pan-Slavism sprang from the teachings of the German historians and politicians, who emphasized the fact of the absorption of Slavs by Teutons in northern Prussia and of Finns by Slavs in the northern part of European Russia centuries ago. Overlooking the fact that this absorption resulted from peaceful intercourse and unconscious assimilation, these German writers began to agitate in favor of Germanizing non-German nationalities by violence. Under the influence of this propaganda appeared Pan-Slavism.

It is believed that the desire to denationalize other nationalities rises from the economic interests of the ruling nationality, or rather of its ruling classes, for the differences in nationality handicap the expansion of trade and business. The results of the efforts to crush weaker nationalities have been negative, as bitterness, hostility and opposing force have been created. The problem of nationality can not be solved by violence.

There are three philosophical doctrines dealing with the problem: cosmopolitanism, emphasizing the unity of mankind and ignoring nationality, or opposing it; nationalism, ignoring the unity of mankind, believing in the separation of one nationality from another and holding

one's own nationality to be the highest, with a special mission in history (Messiahs, Kultur, etc.); and internationalism, holding that all nationalities have equal rights for existence. Self-determination of nationalities is a principle of internationalism. When this principle is realized, the growth of peaceful intercourse and voluntary assimilation of nationalities will be secure—a step forward in the progress of mankind.

FRANCES DENSMORE, *Secretary*

ANTHROPOLOGICAL NOTES

A NOTE ON TWO STONE OBJECTS FROM SOUTHERN BOLIVIA

DURING a recent visit to La Paz, Bolivia, I was shown the two stone objects here illustrated (Fig. 22). They are at present the property of John Davis O'Rear, Esq., Minister from the United States to Bolivia. With about sixty other stone objects of the same sort, these were found by some railway engineers in a large mound in the region of Oruro, Bolivia. The larger of the two is about three feet in height. The material is reddish



FIG. 22.—Stone objects from Southern Bolivia.

sandstone. So far as I have been able to discover, these objects are in no way connected with any of the recognized prehistoric cultures of the region in which they were found. What they are intended to represent, and what their purpose may have been I cannot imagine. The neck or stem at the base is comparatively thin, and it may have been utilized as a means of lashing the heads to poles. This, however, is merely a

conjecture, and the weight of the stones would preclude their having been carried about, although they may have been set up in a stationary position, after the manner of totem-poles and similar things. Perhaps some of the readers of the *American Anthropologist* can furnish a key to the mystery.

PHILIP AINSWORTH MEANS

MUSEUM OF THE AMERICAN INDIAN, HEYE FOUNDATION

PROF. MARSHALL H. SAVILLE has recently returned from an expedition to Guatemala in the interest of the Museum of the American Indian, Heye Foundation. The Museum has been conducting archaeological work in Guatemala since January, 1917. During the latter part of the year the field work was under the direction of Mr. Paul Henning, formerly of the Museo Nacional of Mexico, and later assistant inspector of monuments in southern Mexico. Mr. Henning's researches were conducted along the Pacific coast of the republic, especially in the region of Costa Cuca. Important collections have also been obtained from the southern coast regions, where the little-known Szincas still maintain small settlements. An extensive manuscript grammar and vocabulary of the Szinca language was procured for Mr. C. P. Bowditch, of Boston, who is having it reproduced by the photostat process. Owing to the unusually severe earthquakes which destroyed the city of Guatemala, the stratigraphic work which had been planned in the valley has not been accomplished. In the important work of obtaining archaeological material from the various culture centers of Guatemala, the Museum has had the assistance of the President of the Republic, Don Manuel Estrada Cabrera. The Museum is also having excavations made in various parts of British Honduras under the immediate field direction of Dr. Thomas Gann, and interesting results have already been obtained.

Mr. F. W. Hodge proceeded to New Mexico late in May to direct the excavations to be made by the Harmon W. Hendricks Expedition at Hawikuh, the ruined Zuñi pueblo at which successful work was conducted last summer.

Excavation of a village-site at Throggs Neck, within the city limits of New York, is among the archaeological activities of the Museum. The visible remains at this site consist of a shellheap of considerable extent which is in process of thorough examination under the immediate supervision of Mr. Alanson B. Skinner, who already has found numerous aboriginal artifacts indicating the prehistoric occupancy of the place.

Mr. M. Raymond Harrington has resumed his investigations of the ancient village-site at Croton Point on the Hudson, opposite Ossining, which covers an area of many acres. Mr. Harrington has devoted more or less attention to the study of this site for a number of years, and has procured numerous objects of stone, earthenware, bone, and antler, of the kinds typical of the region.

AMERICAN MUSEUM

DR. HERBERT J. SPINDEN left late in March for an extended trip in Colombia, South America. He hopes to make an extensive survey of the archaeological remains of Colombia and carry on ethnological investigations as opportunity affords.

Dr. Spinden has recently been awarded a prize of 5000 francs by the Angrand Foundation of the Bibliothèque nationale of Paris. The award was for the best work in American archaeology, ethnology, or linguistics produced during the years 1913-1918. It is assumed that Dr. Spinden's memoir entitled "Maya Art," issued by the Peabody Museum of Harvard University, was the work considered in the bestowal of this honor.

Mr. Leslie Spier left New York on the twentieth of May to make an examination of the archaeological ruins north of the Salt River, Arizona. If time permits he will undertake ethnological work with the Walapai Indians.

Mr. Earl H. Morris returned to New Mexico early in June to resume work on the Aztec Ruin. If conditions are favorable it is hoped that a considerable part of the ruins still unexcavated may be cleared during the present summer and autumn. Mr. Morris is soon to be joined by Mr. B. Talbot B. Hyde who will assist in the oversight of the work.

UNIVERSITY MUSEUM

THE University of Pennsylvania Museum has dispatched an expedition to South America under the leadership of Mr. Theodoor de Booy, Assistant Curator in the American Section of the museum, to explore the Sierra Pareja range of mountains in Venezuela not far from Lake Maracaibo. This high range of mountains which juts into Colombia is unexplored and the character of its natives unknown.

Mr. H. U. Hall, Curator of General Ethnology, is serving with an infantry regiment in France.

Dr. Stephen Langdon, Curator of the Babylonian section, is in England serving with a home defense regiment.

Dr. Stephen B. Luce, Curator of the Mediterranean section, has received a lieutenant's commission, junior grade, in the navy and is stationed in Washington.

Mr. C. W. Bishop, Curator of the Oriental section, recently returned from one year of field work in the interior of China.

The Museum is installing a most interesting collection of Moham-medan art which will be opened to public view May 15th.

THE Bureau of American Ethnology has on hand a limited number of copies of Major Powell's articles on Technology, or the science of industries; Sociology, or the science of institutions; Philology, or the science of activities designed for expression; Sophiology, or the science of activities designed to give instruction. Copies of these separates, which were originally printed in the Twentieth Annual Report can be obtained by application to the Bureau of American Ethnology.

MR. NEIL M. JUDD has recently visited many of the ruins in the Kaibab National Forest in northwestern Arizona and that portion of the Grand Canyon National Monument north of the Colorado River. The trip was an archaeological reconnoissance.

MR. JUDD entered military service as an aviator about June first, an extension of time having been granted him by request of Secretary Walcott of the Smithsonian Institution.

MISS MARJORIE A. MALLORY, a member of the Association, sailed for France early in June to undertake secretarial work for the Y. M. C. A. Miss Mallory for nearly two years assisted in the editing of the *American Anthropologist*, and during the last year was chiefly responsible for the form and details of the publication.

DR. T. T. WATERMAN has been selected to inaugurate the anthropological work of the University of Washington at Seattle. Dr. Waterman is to be Associate Professor of Anthropology in the Department of Social Science. A small fund has been established for the furtherance of research in anthropology.

AT the last meeting of the Anthropological Society of Philadelphia, March 21st, Dr. A. L. Kroeber of the University of California spoke on "The Superorganic."

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SBETETDA'Q, A SHAMANISTIC PERFORMANCE OF THE COAST SALISH¹

By HERMAN K. HAEBERLIN

THE shamanistic ceremony which is the subject of the following description was formerly performed by all or most of the Salish tribes on Puget Sound. It has not taken place for a number of years, and I was not able to witness a performance. Information in regard to it has been gained from the descriptions of informants, one of whom had himself been a shaman of considerable reputation. I secured first-hand accounts of the performance from the Snohomish, the Puyallup, and the Squalli. My informants told me that all the surrounding tribes,—the Snuqualmi, Dwamish, Suquamish, etc.,—also performed the ceremony. From some rather casual remarks of the missionary, Myron Eells, it appears that it also existed among the Twana or Skokomish Indians on Hoods Canal. The Skagit, Snonomish, Lummi and all the tribes farther to the north did not perform the ceremony.

The purpose of the *sbetetda'q* ceremony was to regain the guardian-spirit of some person from the land of the dead. Under certain circumstances it was supposed that a person's guardian-spirit had been taken away by the ghosts and had been carried to the land of the dead. If the spirit was not regained, the person would soon die. The ailment of the person bereaved of his guardian-spirit would be psychic rather than physical. It consisted in a general feeling of indisposition. Furthermore, gradual loss of property was an important symptom. If the spirit was not regained, the person

¹ Paper read at the meeting of the American Ethnological Society, on November 26, 1917.

must die. The patient would arrange a performance for himself and would hire shamans for the purpose. A person who dreamed about having been with the dead would also arrange a *sbEtEtta'da'q*.

The thing taken away by the ghosts was not the soul of a person in the general sense of the word, but rather his guardian-spirit.

The idea of the possession and control of guardian-spirits is one of the most prominent features of the cultural life of the Puget Sound Salish. There are a considerable number of such spirits that are acquired by men as well as women in youth. Some of them are very powerful and are obtained only by the most exceptional individuals, others are much weaker and are common to most members of the tribe. There are two distinct types of guardian-spirits among these people. The one type is called *x̣'dā'b*. These are shamanistic guardian-spirits. There are a number of different *x̣'dā'b*, but they all refer to the power of healing. The other type of guardian-spirits are called *sklā'letut* and are profane in the sense that they do not give to a person shamanistic power, but help him in gaining riches of all kinds, *i. e.*, by giving him luck in gambling, hunting, fishing, etc. The patient of the *sbEtEtta'da'q* ceremony has lost his *sklā'letut*, not his *x̣'dā'b*. However, the shamans whom he hires to regain his *sklā'letut* are *dux̣'dā'b*, *i. e.*, people with the other type of guardian-spirit, namely *x̣'dā'b*. But not any kind of *x̣'dā'b* guardian-spirit enables the shaman to partake in the *sbEtEtta'da'q* ceremony,—he must own specifically the so-called *sbEtEtta'da'q x̣'dā'b*. This *x̣'dā'b* alone makes it possible for a shaman to visit the land of the dead and to look there for the lost *sklā'letut*. Those who have attained this power may bring back any kind of *sklā'letut*.

The *sbEtEtta'da'q x̣'dā'b* is said to travel about in a canoe. There are five painted boards (*swan'c*) on each side of his canoe. Ten men form the crew. The headman, who stands in the bow of the canoe, is the *sbEtEtta'da'q* spirit. The canoe was encountered by fasting youth, but only in stormy weather. It would approach the boy who was standing on the shore. The boy must not show any fear, but rush into the water towards the canoe and seize hold of the leader.

The peculiarity of the *sbETetda'q* power consists in that it does not affect cures by sucking, rubbing, or similar magical devices, as is the case with other *x̣^udā'b*, but that the cure is brought about by a dramatization of the regaining of the *sklā'letut*. Furthermore, this *sbETetda'q* power necessarily presupposes the coöperation of a number of shamans all of whom must be in possession of this particular *x̣^udā'b*. Individual action is not possible as in the case of the working of other *x̣^udā'b* guardian-spirits.

The *sbETetda'q* ceremony was performed by an even number of shamans, usually eight in number. According to other informants there might be six, eight, ten, or twelve. It is also said that the number depended upon the number that the person who gave the ceremony was able to hire. Since in one tribe there were never as many as eight shamans who had the *sbETetda'q* guardian-spirit, it was invariably necessary to hire such shamans from neighboring tribes in addition to those in the tribe of the patient. Thus the ceremony was bound to be an intertribal affair. In this connection it is very interesting to note that not all tribes could coöperate in the shamanistic performance. When a Snohomish gave a *sbETetda'q* ceremony, shamans from such allied tribes as the Snuqualmi, Skokomish, Sdohobc, etc., would participate. But he would never hire a *sbETetda'q* shaman of the Dwamish or the Suquamish, although the latter tribes lived in closest proximity and were linguistically just as intimately related as the other neighboring tribes. And *vice versa* a Snohomish *sbETetda'q* shaman never took an active part in a *sbETetda'q* ceremony of the Dwamish or Suquamish. He might be present as a spectator, but he would never be one of the acting shamans. The underlying idea of this grouping of tribes with the Dwamish and the Suquamish on the one side and the Snohomish, Snuqualmi, Skokomish, etc., on the other seems to have been that each group had its own land of the dead, or a different trail leading to the land of the dead, and that therefore a *sbETetda'q* shaman from the one group could not assist in regaining the *sklā'letut* of a patient from the other group. The reason was certainly not a feeling of hostility between the two groups. However, the shamans of one group did not form in any sense a society.

The ceremony always took place in midwinter, either in December or January (according to one informant only in January), and invariably at night-time. The Indians say that the seasons and also the times of the day in the land of the dead are exactly opposite to what they are in this world. When it is midwinter here, it is midsummer there, and when it is night here, it is daytime there. Therefore the most advantageous time to visit the land of the dead is during a night in midwinter, because then it will be a fine, bright summer day in the other world. In fact this is the only time of the year when the trail to the ghost-land is at all passable. This is the reason that the Indians give for performing the ceremony only at night in midwinter.

The performance takes place in a house. There is no special ceremonial house set aside for it. Any house that lies in the direction of east and west will serve the purpose. The Puget Sound

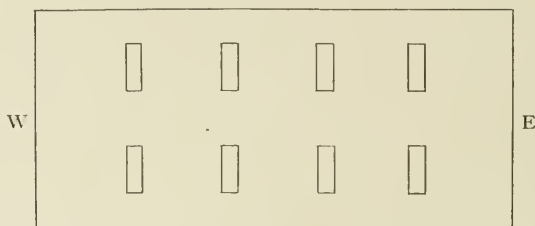


FIG. 23.—Arrangement of dance space.

Salish lived in long rectangular wooden houses that were occupied by a number of related families. If the house of the patient himself did not lie in the proper direction of east and west, then he would rent another dwelling-house that did. The reason why the ceremony had to take place in a house standing east and west was that the land of the dead was thought to lie due west. When the shamans dramatized their journey to this land, they had to face the west and when they dramatized the return journey, they had to face the east. The arrangement of the shamans in the house was as shown in figure 23.

They always stood in two parallel rows. They all faced in the same direction. Beside each man was a magical board called

swan'c. This board was made of cedar. It was owned by the shaman and represented the particular supernatural experiences that he had had when acquiring his *sbEtEtDa'q* guardian-spirit. The form of the boards differed somewhat among the various tribes. Those of the Suquamish are of the form shown in figure 24*a*; those of the Snohomish have a round head (fig. 24*b*). The lower part of the board was stuck into the ground in a hole dug for the purpose so that the board stood erect. Each shaman also held in his hand a pole six to eight feet long (*tsk!ō'sEd*), which referred to his guardian-spirit, and which was worked up and down during the song and dance. During the ceremony he stood in front of his board pointing his pole to his sign on the board.

The two parallel rows of men invariably represented the crews of two imaginary canoes. The man at the head of each row was the leader of the party in his particular canoe. He began the songs. The man at the end of the row was supposed to steer the canoe. Besides, each shaman worked his magical pole as if it were a paddle.

A particular kind of song belonged to each one of the guardian-spirits. And the *sbEtEtDa'q* spirit also had its song. When the shamans were dramatizing their journey to the land of the dead, they sang this song that they had learned in their youth from the *sbEtEtDa'q* guardian-spirit. A large audience of spectators sat around the sides of the house and accompanied the shamans in their songs. It seems that each shaman sang his own song in turn. The different *sbEtEtDa'q* songs of the various shamans were not quite the same. It is said that the spectators sang "to lift up the shamans."

The trail which the shamans had to travel over was beset with many difficulties. It was the same trail that the soul of a deceased person had to travel over in order to join the ghosts. The soul of the dead traveled along this trail on foot, not in the burial canoe. As I have already said, the land of the dead was thought to lie in the west. While the trail that led to it descended, I was told ex-

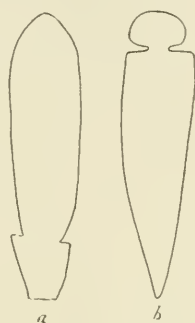


FIG. 24. — Outlines of boards; *a*, that of the Suquamish; *b*, that of Snohomish.

pressly that it was not below this world in the sense of being an "underworld," but that it was on the same level with this world. The shamans have to cross two rivers to get to the land of the dead. The first one is exceedingly swift and cannot be crossed in a canoe. The departing souls of the dead cross it by walking over a tree that has fallen across the river. The performing *sbEtEtDa'q* shamans dramatize the passage of it by laying their medicine poles on the ground and then walking from one end of these poles to the other, as if they were in this way crossing the river. Since the poles are narrow and round, the shamans must take great care not to let their feet slip off and thus touch the ground. If this should happen to one of the shamans, it would be a great calamity to him as well as to the whole expedition. It meant that the shaman had slipped into the river. The Indians claim that the feet of such a shaman would at once swell up and that he could not walk. He became a burden to the whole expedition, since his colleagues could not abandon him, but had to support him. This added a new task to the work of the shamans, which was supposed to be sufficiently difficult in itself.

After traveling on, always in a westerly direction, the shamans arrived at the second river. This one was much broader than the first one and flowed much slower. The shamans crossed this river in an imaginary shovel-nose canoe. It was at this point in the ceremony that they worked their magical poles as if they were paddles. The eastern approach to this river was flat, but on the opposite side there was an embankment. It was on this embankment, just above the river, that the village of the dead was located. Their mode of life and the form of their village corresponded in everything to the life of this world. The men hunted and fished and the women performed the same kind of work as here. The essential difference between the two worlds is that the seasons are always opposite, and when it is night here, it is daytime there. Furthermore, when it is low tide here, it is high tide there. The inhabitants of the land of the dead are called *skayū'*. They live in houses similar to those of the people of this world. They walk with crossed legs. They have canoes and go out fishing and hunting.

They are always trying to steal off with the souls of living persons and to take them away to their ghost-land. For this purpose they hover about the dwellings of the living and try to steal things belonging to the living. When they have succeeded in stealing a sufficient amount of property belonging to a certain person, the latter is bound to die. Against this influence of the *skayū'* the Indians take many precautions. When a person dies, they are careful to dispose of all the belongings of the deceased, either by putting them into his burial canoe or by burning them. If they did not do this, the ghost would hover about his old habitat and cause others to die.

While on their journey to the land of the dead, the shamans would sometimes meet a *skayū'* who was out picking berries. This *skayū'* was impersonated by an Indian, who walked with crossed legs and made peculiar gestures and grimaces. The shamans tried to get information from him regarding the lost soul. When the village of the *skayū'* was reached, the latter were unwilling to give up the guardian-spirit that the shamans were looking for. So a fight between the shamans and the ghosts was bound to ensue. This was dramatized in the following way. Boys who stood at the west end of the house represented the fighting *skayū'*. They shot off burning cedar splints.¹ The boys did not shoot these directly towards the shamans, but rather towards the ceiling of the house. But if a burning splint happened to strike a shaman, he would at once fall down and stop singing and dancing. The limb that was struck by the dart swelled up. The other shamans at once undertook a magical treatment of their afflicted comrade, and they would support him on the return journey. As soon as the shamans had secured the lost guardian-spirit of their patient, they set off on the return to the country of the living. The retreat was carried out strategically, as they constantly had to fight rearguard actions with the pursuing *skayū'*. If one of the shamans had been wounded by one of the burning arrows, others would fight the *skayū'* to protect him from falling into their hands, while still others supported the invalid. The Indians claim that if a burning arrow struck the head

¹ In the same way ghosts may be driven away by shooting at them burning cedar darts, of which the *skayū'* are afraid.

or any other vital part of the shaman, it might lead to his immediate death. After their return, the shamans "blocked the trail" so that the ghosts could not follow them into the land of the living. I do not know how this was done.

When the shamans had secured the guardian-spirit of their patient, they made a motion towards the *swan's* boards as if they were putting them into their canoe. As soon as they dramatized their return to the land of the living, they changed their positions and faced the east. When the shamans had succeeded in getting back to the land of the living, they pretended to hold in their hands the guardian-spirit of their patient. Then they began to sing the song of this guardian-spirit. When the patient heard his song, he knew that his lost guardian-spirit had been regained. He would then begin to dance and sing his own guardian-spirit song himself. This marked the end of the work of the shamans.

The shamans were paid by the patient who also gave presents to the spectators who helped in the singing. When a poor person was unable to give a *sbEtEtda'q* ceremony, a relative who was a *sbEtEtda'q* shaman might perform the ceremony for him free of charge, and also the other participants. Or a poor man might pay for the ceremony by giving his daughter in marriage to a shaman. Then the latter did not give anything in return for the woman. A poor person might also promise to pay the doctors as soon as he acquired some property.

If the sick person did not get up and dance, it meant that the shamans had not brought back the right guardian-spirit. In that case, the shamans had to return the payment which they had received at the beginning of the ceremony. Ordinarily the spectators distributed presents to the shamans at the close of the performance.

One informant told me that when the shamans dramatized their return, a spectator would occasionally hear his song sung and would get up and dance. This signified that the guardian-spirit of this particular person had been in the land of the dead, in spite of the fact that the person was not conscious of it and that it had now been brought back by the shamans. This corresponds in a certain respect

to the fact that occasionally a *sbETetda'q* ceremony might take place without the initiative of a sick person. Under such circumstances the shamans arranged a ceremony on their own initiative, and traveled to the land of the dead with the purpose of finding out whether the spirit of any living person had been stolen by the *skayū'*. If they found any, they would bring it back. Under such circumstances the shamans were paid for their services by the persons whose spirits had been regained, even if they had not been hired for the express purpose.

Among the Snohomish the *sbETetda'q* ceremony lasted either one night only or two nights. In the former case the fight with the *skayū'* dramatized by the shooting of the burning arrows took place at midnight. Before midnight the shamans faced towards the west, after it towards the east. My informant volunteered at this point the remark that the position of the shamans was determined by the course of the sun. There does, indeed, seem to be a certain association between the course of the sun and the journey of the shamans to the world of the dead. Such associations are naturally hard to get at.

If a *sbETetda'q* ceremony lasted two nights, then the first night was taken up with a dramatization of the journey to the ghost-land, the second night with the return to the land of the living. In that case, no performance took place during the daytime between the two nights. The shamans slept during the day.

Among the Squalli and the Puyallup the *sbETetda'q* ceremony seems to have been usually of a longer duration than among the tribes farther north. At least my informants always spoke of from five to six days.

PRINCIPLES OF ESTHETIC FORM IN THE ART OF THE NORTH PACIFIC COAST

A PRELIMINARY SKETCH

BY HERMAN K. HAEBERLIN

IN so far as esthetics is not merely a fanciful structure of metaphysical postulates, but deals with demonstrable relations, it is a study of the artistic principles characteristic of a certain cultural group or epoch, or of a certain school of artists. The very fact that such principles can be found demonstrates the cultural significance of the phenomena in question and gives to the science of esthetics a place in the series of cultural sciences. If artistic expressions were individualistic in the sense that they were *disjecta membra*, we could indeed not speak of principles. But everywhere in cultural growth do we become conscious of broad underlying laws which we abstract from the individual phenomena by conceptualization and which make a scientific study of cultural phenomena possible beyond the stage of pure description. This seems to be the essential trait of culture history in contradistinction to individualistic history in which we deal with the succession of dynasties, the fates of armies, and the intrigues of statesmen. Thus from a culture-historical point of view we study the principles which underlie the art of a certain cultural group, how in the progress of cultural development the principles become gradually metamorphosed into new ones, and how the whole presents the continuity of organic growth.

All of our culture-historical concepts, such as culture areas, cultural specialization, assimilation of cultural borrowings and the like, are based on the existence of principles by which a cultural area or a cultural epoch may be characterized. But more than that we gain even our norms of cultural evaluations from the conception of such principles. For surely we do evaluate when we

speak of cultural centers and of the fringes of cultural areas. We actually do place the stress accent on the cultural centers and not upon the marginal areas. Purely descriptively there is no reason why the marginal areas should be of less significance or popularly speaking "lower" in degree of cultural development than the nuclei of the areas with their highly specialized cultural forms. We place the culture centers there where we discern broad principles underlying the complexity of cultural life. It is illusory to believe that a purely descriptive culture history is possible.

Our study of underlying cultural principles may be either extensive or intensive. If it is extensive, we investigate the cultural life of a people as a whole, we correlate its different phases, study their associations one to another, and determine the common ideas which dominate them. But if our study is intensive rather than extensive, then we abstract a certain phase from the other phases and study the principles involved in the relation of its elements.

In the following I propose to consider a sphere of culture-historical study of the intensive type to which very little attention has been paid. It consists in a study of the relations of form in the art products themselves. When these relations are such that we recognize them as typical of a certain culture area, we call them stylistic. The typical nature of these relations of form is the essential point, because only in so far as phenomena are typical do we speak of culture-historical principles. Most of those erratic markings which have been collected as the "*Anfänge der Kunst im Urwald*" seem to be equally void of culture-historical bearing as are the helpless scribbles of children on which so much interpretative energy has been spent.

In the case of the carving and painting of the northwest coast we are dealing with an art whose style is "felt" by every one. To whatever objects this art is applied, be it to totem poles, house fronts, canoes, dishes, or spoons, we are always confronted by certain characteristic features of style. Certain characteristics of this art have been discussed. For instance, attention has been called to the fact that invariably the whole of the animals represented is given in the carving or painting, no matter how dispro-

portionate the size of the different parts of the body may be. Furthermore, it has been pointed out that in order to make such an entire representation possible the device of showing the animal "unfolded" either along the front or the back has been resorted to.

These important principles refer still to the contents of the representations of this art, not really to the relations of forms, for which it would seem to me the term "artistic" is properly reserved. Such problems, for instance, as the relation of the forms of the mouth of the crest animals to the form of their eyebrows, if investigated, would reveal principles of "Formgebung" which might be as feasible of demonstration as is the principle of "unfolding." The mouth of the animals is either quite horizontal or its corners are turned down very considerably. Similarly, the eyebrows either lie about in the same plane or are considerably on the slant. Even a superficial survey I think is sufficient to impress one with the correlations which exist between these different forms of the mouth and the eyebrows. Similar problems of the artistic relations of lines arise in the case of the relation of the eyes to the superimposed ears of the animals and of the beak or nose to the rest of the face. Furthermore, such phenomena are of interest as the curvature of surfaces and the persistency with which painted lines are given artistic "character" by making them lighter and heavier at different points, as for instance in the outlines of the eyes which represent joints. By such a method of analysis we should arrive at the formulation of a number of esthetic principles which underlie the art of the northwest coast and which have thus far only been "felt." Only after the definite formulation of such principles can we attempt a scientific comparison of the artistic qualities in the style of different cultural groups.

We should study not only the relations of lines and surfaces within the individual figures represented. An equally important task is to ascertain the principles which underlie the artistic combination of the different figures of a totem pole, of a spoon handle, or the like. A hasty survey reveals devices of composition that are in principle the same as some employed in our own art. For instance, a composition of crest animals is sometimes effected by

having a line in one figure continue in a succeeding one. In such a case the function of the line may be different in the two figures as far as the content is concerned, but from the point of view of form the continuity of the line results in a pleasing harmony in the outline of the figures represented. Another device of composition is the relation into which the projecting ear of a figure is brought with the figure above it. This relation is often worked out with great skill and presents an example in which the primitive artist solved an esthetic problem of composition,—a problem consisting in this case in the bold composition of the ear of the lower figure with the legs and haunches of the one above. Under this heading of the relation of different parts of a totem pole to one another occur some of a very subtle nature. For instance, in some memorial columns on the top of which a bird is placed, the curvature of the back of this animal and the outline of its wings appear to be adjusted with a wonderful feeling of form to the rigid vertical lines of the undecorated section of the column.

The most striking demonstration of the esthetic sense of the northwest coast artist lies in the adaptation of his subject matter to a given surface. There are a great diversity of surfaces to which he must adjust his composition. The totem poles offer cylindrical surfaces, the handles of spoons horn-shaped ones, dishes are round or oval, and canoes and the fronts of the houses have again different shapes. The given surface is the primary condition of composition and its utilization as an esthetic factor presents to the artist ever new problems. The solution of these problems involves truly artistic imagination. To speak here simply of technical mastery is not right. If this term should have any meaning whatever in our study of art, it must be made to refer strictly to the automatic motor habits which are bound to result from specialized activities. Certainly it must not vitiate the concept of artistic imagination, which is as indispensable in the study of primitive art as in that of our own. The adjustment of the same designs to different given surfaces is one of the most fruitful fields for studying the effectiveness of this imagination. The criteria are the different form-relations of the same design on different kinds of surfaces. Such com-

parisons I think can be demonstrated in concrete terms. In the art of the northwest coast comparisons of this kind are rendered especially feasible on account of the conventions of a non-artistic character by which the artist is bound to represent all parts of an animal as well as never to omit certain characterizing features (for instance, the cross-hatched tail of the beaver, the long beak of the raven, etc.) whatever the shape of the surface may be.

To summarize briefly there are then three distinct lines of research in our study of principles of form:

1. The study of the principles which underlie the formal relations of the different parts of the animal figure to one another;
2. The study of the principles of the formal combination of successive figures;
3. The study of the methods of composition with reference to a given surface.

The pursuit of these lines of investigation is bound to lead us to a more exact determination of what constitutes the style of the northwest coast art. Our study will attain culture-historical depth by comparing the stylistic form relations of the cultural center with those of the marginal areas, or probably just with the absence of such form relations. Certainly the gradual waning away of the principles of esthetic form, which are valid among the Haida, as we proceed southward to the Kwakiutl and finally to the Nootka and Salish tribes, and northwest to the northern Tlingit and southern Eskimo, is very instructive.

The relations of forms, the analysis of which is urged here, will be of varying degree of demonstrability. Some relations like the continuity of lines in different figures will be directly and geometrically demonstrable, while others will be more or less recondite. And there are even bound to remain such relations which can only be "felt" and to which the student of art can only call attention in order that others may experience them. But certainly such a condition of affairs is in no way characteristic of the esthetics of primitive art. The study of our own art is confronted by the same situation. For example on the one hand we may study the spatial relations of a Gothic cathedral in the purely mathematical terms of

the relation of the intercrossing of the longitudinal and the transversal naves (= das Quadrat der Vierung) to the other dimensions of the edifice; on the other hand no one doubts the legitimacy of studying the purely psychic relations inherent in the different elements of a modern piece of art. It is well known, for instance, what wonderful use Rodin makes of the human hand for purposes of characterization. In his sculptures the relation of the hand to the rest of the figure may be indispensable for the unity of artistic conception and still, in spite of this innateness, a pragmatic demonstration of this relation would be inconceivable on account of the purely ideological nature of the relation. The result is that the relation can be only experienced intuitively. It must be "nacherlebt." The more spiritual the synthetic unity in art becomes, the less are the relations pragmatically demonstrable.

The only plea I wish to make is that we study the formal principles in primitive art by methods comparable to those applied in the esthetics of our own. We are likely to look on primitive art simply as an ethnographic element and to limit our study to its relations with the other elements of a cultural unit. This I have called the extensive line of research. By an intensive study of primitive art we become conscious of the essential identity of problems in primitive art and in our own. Surely both lines of study may become mutually helpful. The study of primitive art has the great advantage of an ethnological perspective in which the cultural relations, I mean borrowings, assimilations, specialization of cultural elements, are far more plastically outlined than they are in the history of our own art. On the other hand the esthetic study of our art is privileged by being able to become individualistic and biographical, so to say, thanks to the detailed documentary evidence bearing on its historical development. It is true that this may become an evil when the student is not able to look beyond the historical details and to see the broad underlying principles of cultural relations. But in the study of primitive art it is just this biographical feature of the history of modern art that we need for stimulation. We tend too much towards conceiving the art of a primitive people as a unit instead of considering the primitive artist as an

individuality. It is necessary to study how the individual artist solves specific problems of form relations, of the combination of figures, and of spatial compositions in order to understand what is typical of an art style. A purely ethnological point of view in the study of primitive art is inadequate. We need a broader culture-historical outlook. It may seem paradoxical, but it is nevertheless true that ethnology becomes the more scientific, the more it forgets that it is a science. Ethnology is a fortuitous unit. It is the culture-historical point of view that counts.

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A POINT OF GRAMMAR AND A STUDY IN METHOD

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THE origin of the Melanesian and Polynesian pronouns is really a problem for the specialist; in itself it holds neither interest nor profit for any but scholars in those languages. The wider public cannot be plagued with small details: it awaits the more vital conclusions and leaves the experts to discuss the minute demonstrations that lead up to those conclusions.

Through some accident however these pronouns have caught the eye of the philosopher who thought to find in them proof and illustration of a psychological doctrine. Following his example the culture-fusionist seized upon them to support a different view altogether. Thus by luck they have attained to a theoretical importance to which intrinsic merit scarcely entitles them.

Few problems as concrete as this one have been treated both by the psychological and by the culture-fusion schools. These pronouns provide us therefore with an excellent touchstone of the methods and assumptions of both schools; the more so as the material is linguistic; for our information about languages is vastly more detailed than about customs and beliefs, and moreover language has long been subject to an exactness of treatment to which no other branch of ethnology has yet approached.

Such are my reasons for appealing in this paper to the ethnologist in general and for hoping that he will patiently labor through details of grammar that do not interest him, for the sake of methods and principles that do.

THE PROBLEM

One peculiarity of these possessives is the cause of all the trouble. We Europeans have but one series of possessives: my, thy, his, etc. In Melanesian and Polynesian there may be as many as five. We use the same possessive whatever the nature of the possession; they distinguish various modes of possession. Thus we say "his

leg," "his house," "his food," "his drink." The Fijian uses a different word each time, as who should say "legis," "nis house," "kis food," "mis drink."¹ Now this seems a very cumbrous arrangement, Heath Robinsonian grammar, as it were, an elaborate apparatus to produce no more result than we effect by simpler means.

THE PSYCHOLOGICAL THEORY

This complication is a defect, says the psychologist; a defective language argues a defective mind; therefore the South Sea mind is defective: it lacks the power of abstraction; it cannot distil out of experience the pure idea of possession, but at most only extract crude, impure notions of possession mixed with foreign matter. The savage mind can conceive the possession of a leg, the possession of a house, the possession of a drink; it cannot conceive possession pure and simple.

I will not discuss this theory at any length here, as I have done so in my article on "The Psychological Interpretation of Language."² I will merely sum up the arguments.

Firstly it is impossible to judge a people by their grammar, for their language is seldom entirely their own, being oftenest borrowed in part or wholly from other peoples. Kindred races often speak widely different languages, whereas the same language may be shared by races remote from one another in physique and character. The past history of a language influences its present. The conditions in which people live also account for a great deal in their speech. For language is indeed the creation of mind, but of mind working upon past traditions and present environment; we cannot understand how that mind works unless we know what it works upon.

Secondly, the very assumption is wrong from which the psychological theory starts. The Melanesian and Polynesian possessives are not multiplied beyond need, but every one is indispensable. In the examples selected by the psychologist to illustrate his theory one possessive would do just as well as three or four; but we have no right to judge an idiom by a few examples

¹ *yavana, nona vale, kena kakana, mena wai.*

² *British Journal of Psychology*, vol. v, pt. 3, pp. 267 ff (Nov., 1912).

picked out at random. On that principle it would be equally possible to prove that English has a superfluity of prepositions, that "in," "on," "at," could have their work done just as effectively by a single preposition. We must see an idiom at work before we can appraise its utility. On closer acquaintance these pronouns appear as fulfilling a most important function, as expressing distinctions which we also are compelled to express, though we do it by other means: what we convey by varying our prepositions they sometimes convey by a change of possessive.

Thirdly, direct observation tells us that these savages, so called, are perfectly capable of expressing abstract ideas at least equal to that of possession in general.

THE CULTURE-FUSION THEORY

The culture-fusion theory admits that a people's mind cannot be measured by their language. It does take into account the accidents of history and the force of environment. But it accepts the assumption from which the psychological theory takes its start. It tacitly admits that these possessives are unreasonably numerous; it does not once entertain the idea that they may be a logical and systematic growth, even as our genders are, or our conjugations, or our prepositions. If they are not the outcome of mental deficiencies, neither are they the product of sense; their apparent foolishness must therefore be the result of accident; and this is how the culture-fusionist conceives this accident. Let us suppose two cultures X and Y; X includes *a* and Y includes *b*. If those two cultures come together they will fuse into a compound XY which will include both *a* and *b*. Thus given an X folk that use the pronoun "I" alone, and a Y folk that instead use only "me"; the people resulting from the fusion of X and Y will use both *I* and *me* to represent the first person singular. In the Pacific it is supposed that one people said "nis leg," "nis house," while another people said "legis," "houseis"; that both peoples became one, preserving both modes of speech, using sometimes one, sometimes the other, saying "legis," but "nis house," "handis," but "nis hat." Such is the gist of the theory propounded by Dr. Rivers in his *History of Melanesian Society*, vol. II, p. 488.

To begin with, the theory has somewhat simplified the facts: the Melanesian possessives number not two, but many. Modern Fijian has four: *-na*, *nona*, *kena*, *mena*, which for convenience I have represented in English by “-is,” “nis,” “kis,” “mis.” It once had five possessives, possibly six. To be consistent therefore we should invent at least five peoples to account for the Fijian usage alone, to say nothing of the other Pacific tongues. Let that pass however; for the sake of argument we shall suppose with Dr. Rivers that there are only “two modes of denoting possession in Melanesian”; we shall count the suffixed possessive as one mode, and all the rest together as another, to return to our illustration *-is* will be no. 1, *nis*, *kis*, *mis* can be lumped together as No. 2.

The test of a good theory is that it explains every detail naturally by its own resources, without calling to its aid vain suppositions to fill the gaps. A theory of these possessives should account both for their form and for all the peculiarities of their use.

The culture-fusion theory practically makes no attempt at explaining the form. The second series for instance, the one I have represented by *nis*, is compounded of the first series and a particle *-ne* or *no*. What is the meaning of this particle? Why was it chosen? No answer is offered to these questions.

The theory does make an attempt to explain the use; but the explanation, as we shall see, hardly squares with the facts, even after it has been corrected and readjusted with suppositions for which there is no evidence.

The use of the two series of possessives may roughly, though not accurately, be summed up thus: the suffixed possessive, that which I have represented by *-is*, goes with the names of parts of the body and with kinship terms. Now this is hardly what we should expect from the theory, of which the logical consequence would be that all words derived from the language of the X people should take the suffixed possessive, and that all words of Y origin should have the other possessive. To use an analogy: supposing we retained both the Norman and the Anglo-Saxon possessives in English we should naturally expect the rule to be “his father,” “his ox,” “his house,” but “sa uncle,” “sa beef,” “sa table”; we should not

expect pure caprice. Now it is hard to believe that all the terms for parts of the body come from the X people, and that the Y's supplied all the rest of the nouns.

Dr. Rivers indeed claims to have found a dialect where his theory is verified, where the X words take the suffixed possessive, and the Y words the other one (I, 290, II, 198 and 488). Among the Nandrau tribe in the interior of Fiji there are two sets of kinship terms: one takes the suffixed possessive as among the coastal tribes; the other is preceded by the possessives of the second series; this latter set he supposes to be peculiar to the mountain tribes and concludes that they are an "older linguistic substratum" (II, 199). The argument is: the earlier inhabitants, our Y people, placed the possessive before the noun; the immigrants from the coast, our X's, tacked the possessive on to the noun; the present compound tribe places it before Y words and after X words. Which is what one would expect from the theory.

Unfortunately these supposed indigenous words are nothing of the kind; they are not confined to the interior of Fiji but are common all over; they are derived from universal Fijian roots according to universal Fijian rules.

Secondly they are not true kinship terms at all, but polite circumlocutions such as all Fijians delight in. I will analyze a few of these terms:

Veikila is derived from *kila*, "to know." *Veikila* is "to know each other," "to be acquainted." As acquaintance in Fiji implies kinship it also means "to be related," and hence "to intermarry." Thus when a Fijian from the hills calls his cousin his *veikila* he is merely describing him as one with whose family he intermarries.

I vola is derived from an old hill word meaning "to apportion" by means of the favorite Fijian prefix *i* which has instrumental or passive force. When a man calls a woman his *i vola* he is merely describing her as the woman assigned to him. The coastal Fijians preserve this root in the word *ndavola* which is used for the same relationship, and which is merely the same word with a different prefix; it has there become a real kinship term and accordingly takes the suffixed possessive.

I sa is a common word all over Fiji meaning "vis à vis." As a kinship term it is found in parts on the coast and in the island of Kandavu. It is needless to multiply instances.

Thirdly, contrary to what Dr. Rivers thought, the hill tribes are not alone in replacing kinship terms by descriptive words, though they do so far more extensively and constantly than the coastal people. A Lauan, than whom no Fijian can be said to be more coastal, will inquire politely about "your originator" (*i tum-butumbu*)¹ meaning parents; for your wife he will say "your lady."

In conclusion these Nandrau terms are not indigenous words, but merely examples of the universal Fijian custom of polite speech, which consists in substituting for the true name a descriptive term usually derived from a verb by means of the prefix *i*-.² These terms not being kinship terms do not take the possessive proper to kinship terms.

The culture-fusion theory can scarcely hope to fit the facts considering that it starts from a misapprehension. I said above that roughly speaking the suffixed possessive goes with names of parts of the body and with kinship terms. That is a rough statement, useful in practice at the start, but totally useless as a basis for theory. It is not the case that each series of possessives is restricted to a particular set of words. Few words are confined to one possessive only; many, if not most can be used with two possessives; some with three, a few with all four. A word can take any possessive that is not inconsistent with its meaning. It may be said that in theory a noun may take any possessive, but in practice it is generally limited to one or two out of the four, because the rest have a meaning inconsistent with its own meaning. To understand this the reader must know the meaning of the various possessives which I have represented by *-is*, *nis*, *kis*, *mis*.

No. 1 implies partial identity,

No. 2 means "possessed by," "used by,"

No. 3 means "destined for,"

No. 4 means "drunk by."

¹ From *tumbu*, "to grow," and the prefix *i*.

² In this language the hand becomes "the instrument," the head "the place above," etc.

Now it is evident straightaway that a Fijian can never say "housis" because a house is never part of anybody; nor can he say "mis house" because a house can never be a drink; but he can say "nis house," since houses are owned; he can also say "kis house" when it is intended for a certain purpose. A stem is part of a tree, hence "stemis"; but it may mean the stem of the family, the ancestor, and then it is "nis stem" since it is not part of anyone; "kis stem" and "mis stem," on the other hand, would not fit. Water, liquid, on the other hand can belong to someone or something in all these ways: it may be part of a thing, its juice; it may belong to a man; it may be destined to a certain purpose; and it is commonly drunk. Hence the word *wai* which means liquid, medicine, may take all four possessives according to the meaning, thus

wai-na: its juice;

nona wai: his medicine (the doctor's);

kena wai: its remedy, the cure for it;

mena wai: his drink, his medicine (the patient's).

It may even happen that a word is used with two possessives at the same time. Though very rare this idiom alone is enough to upset the culture-fusion theory.

The assumption therefore that the Melanesian possessives are each assigned to a different set of nouns is not verified by experience; and with it falls the theory based upon it.

But enough of criticism; let us pass to construction. This is not so difficult a task as to *expound* the results. To appreciate the evidence properly some knowledge of Pacific languages is necessary. This can be expected of few readers. I shall be compelled therefore to reduce the evidence to its simplest expression and be content with the barest necessities. Moreover to avoid confusing the reader with a multitude of strange languages I shall confine myself as far as possible to one language, namely Fijian, since it is the basis of the culture-fusion theory and it also supplies much of the evidence. I shall only go outside when necessary.

ANALYSIS OF FORMS

The first step is to tabulate our possessives. In the Mbauan dialect of Fiji they are as follows:

Sing.	{ 1st pers.	-nggu	nonggu	kenggu	menggu.
	{ 2nd "	-mu	nomu	kemu	memu.
	{ 3rd "	-na	nona	kena	mena.
Plur.	{ 1st incl.	-nda	nonda	kenda	menda.
	{ 1st excl. ¹	-ikeimami	neimami	keimami	meimami.
	{ 2nd pers.	-muni	nomuni	kemuni	memuni.
	{ 3rd "	-ndra	nondra	kendra	mendra.

Two dialect forms that represent the second series must be mentioned here as they are of interest. Lasakauan has:

Nenggu, nemu, nena, etc.

In Lauan it runs:

nggou, omu, ona, etc.

In Hawaiian the second series appears in two variants:

no'u, nou, nona, etc.

and

na'u, nau, nana, etc.

Hawaiian also has the Lauan series:

o'u, ou, ona, etc.

We now have enough to start. It is obvious at first glance that the second, third, and fourth series are merely the first attached to monosyllabic particles instead of being stuck directly on to the noun. What then is this first series out of which all the others are compounded?

They have been called possessives. Now this is an idea we must get out of our heads before we proceed any further. They are not really possessives, but personal pronouns; strictly speaking they do not mean *my, thy, his*, but *I, thou, he*. Take the first person plural inclusive, for instance, it can equally well be used as subject, thus:

nda lako: let us go.

When therefore a Fijian says "*yavanda*," he says in effect not "our leg," but "leg we."

Therefore these so-called possessives are really personal pronouns in apposition.

So much for the terminations. Now for the particles *o, ne* or

¹ Inclusive and exclusive, *i. e.*, including or excluding the person spoken to.

no, *ke*, and *me*. If the second part be a pronoun, what is the first likely to be? Answer: an article or a preposition. Both answers are right, for in Melanesian and Polynesian the article and the preposition run into one another. We must reduce proof to its simplest expression.

Lauan series: *O* means "of" throughout Polynesia.

2d series: *Ne* means "of" in High Fijian before proper nouns;¹ in Rotuman before common nouns;

Ni is "of" in Fijian before common nouns;

Na and *no* mean "of" in Hawaiian, Tahitian, etc.

3d series: *Ke* in Fijian means "for" before proper nouns.² In certain dialects it also means "to," "towards" before common nouns;

Ki means "to" in most Fijian dialects, in Tongan, Maori, etc.

4th series: this one is obscure and will be left out of consideration.

The conclusion is thus reached that these so-called possessives are not really possessives, but personal pronouns in apposition to nouns or preceded by prepositions. We may express it otherwise by saying that they are pronouns in various cases.

Andrews saw this clearly in the Hawaiian language.³ Indeed he could not help seeing it. Here is a list of Hawaiian possessives in the 3d person singular:

o na or *ana*: of him,

Kona or *kana*: his,

nona or *nana*: for him.

Not one of the particles *o*, *a*, *ko*, *ka*, *no*, *na*, but is still used as a preposition before nouns to this day.

THE EXPLANATION

The reason why there are so many possessives in Melanesian and Polynesian is that there are none. This may sound paradoxical, but an analogy will make it clear.

¹ This fact is obscured by the usual spelling *nei*. As a matter of fact *i* is the article that always precedes a proper noun in the oblique cases. It should be *a vale ne i Rasolo*, not *nei Rasolo*.

² Usually written *kei*, but *i* is really the article before nouns in the oblique cases.

³ *Grammar of the Hawaiian Language* (Honolulu, 1854), pp. 57 ff.

Supposing we English had no possessives, we should have to supply the lack in some way, probably by means of prepositions. We might then have to say: "the house of him," "the bread for him." Where we now say "his book" we might have to choose between "the book of him" and "the book by him" according as "he" was the owner or the author. Our phrase "his story" would be represented by "the story by him" or "the story about him" as the case might be.

Well that is exactly what the Pacific Islanders have done. And after all we need not invent analogies, for we have done something similar in our own language; compare "my father's house" and "the House of God," "Grimm's tales" and "the tale of Puss in Boots."

AN OBJECTION

I have so far kept silent about one serious difficulty in the way of the theory I have propounded. If the so-called possessives of Melanesian and Polynesian are really nothing but pronouns with prepositions they ought to occupy the same position in the sentence as nouns with prepositions. We have laid it down that in ethnology, as in other sciences, if $A=B$, whatever is true of B is true of A . It is the standard of our own choosing and we must abide by it. Now it so happens that in Fijian and kindred tongues possessives do not behave like prepositions followed by pronouns or nouns; prepositions governing nouns and pronouns follow the principal noun, whereas possessives of all but the first series come between it and the article. According to our theory we should expect Fijian to say:

a vale ne i Rasolo: the house of Rasolo, and

a vale nona: the house of him,

or else

a ne i Rasolo vale: Rasolo's house, and

a nona vale: his house.

In point of fact it says:

a vale ne i Rasolo: the house of Rasolo,

but

a nona vale: his house.

The whole fabric of our argument would collapse but that Hawaiian comes to our rescue once more. In that language the dependent noun may either follow the principal word or come between it and its article, thus

ka hale o ka ali'i: the house of the chief,

or

ka ko ali'i hale: the chief's house.¹

Pronouns behave in the same way:

ka hale o makou: the house of us,

or

ko makou hale: our house.²

We therefore conclude that in the parent language of Polynesian and Melanesian the dependent noun or pronoun could stand either before or after the principal word. The possessives being nothing but pronouns in the oblique cases could take up either position. Fijian in common with many other languages of the same family has lost this freedom of construction.

"Easy, easy," will some one say, "let's not run away with the argument. We know the Hawaiian rule, and we know the Fijian rule; but what right have we to jump to the conclusion that the Hawaiian is the original one? Why should it not be a recent and local development?"

Questions as to which of two usages is the more ancient are usually settled by survivals. Let us suppose two peoples A and B originally derived from the same stock; A practises a custom which B has not; is A keeping up an old custom which B has lost, or has A developed this custom after parting from B? If we can find traces of this custom lingering among the B folk then it is evident that the A's are merely keeping up the traditions of the parent race. In other words whenever such a problem arises we immediately look for survivals.

Here we must be allowed to pause a moment in order to define survivals.

A DIGRESSION ON SURVIVALS

A survival is the corpse of a custom. A custom is living so long as those ideas are living which brought it into being. When those

¹ Andrews, p. 34.

² *l. c.*, p. 82.

ideas die, the custom withers. The ideas are the sap which makes the custom grow, blossom forth and propagate. A living custom changes and expands; a dead one is rigid and unchangeable like a skeleton. Take for instance the English prefix "for" in "forlorn," "forgone;" it is dead; it is a survival; no one can tell its meaning who has not some knowledge of English philology: it persists mainly in a few old-fashioned words which themselves persist in a few old-fashioned idioms and in poetry; it cannot be used at will to make new words, such as "forworn," "forburnt," "forbroken," but the words in which it is allowed have to be learned by heart. Contrast with this the prefix *un-*; it is ancient yet as full of life as ever; the meaning is clear to every child; it can be set before any Anglo-Saxon adjective, provided it makes sense. Give me an adjective I have never heard before and I will compound it with *un-*; in using it I do not obey the dictates of memory but follow the counsels of reason.

What is true of words is also true of beliefs and institutions. The fear of being thirteen at a table is a survival: the underlying principle has evaporated; no one can even guess what it was; nothing is left but the dry bones; it can never increase and multiply. The use of disinfectants on the other hand is a living custom, for the theories at the back of it are still in their prime; they change, improve, and expand, give up old applications and find new ones. Sainthood is a survival among Protestants; for with them it is little more than a title accorded by tradition to some great men of the Church in olden times, but no longer now conferred. Among Roman Catholics it is a living belief which still creates new saints to the present day.

In short a survival is in ethnology what a fossil is in zoölogy.

SURVIVALS AND POSSESSIVES

Let us now apply this conception of survivals to the present problem. In Hawaiian the possessive can either come before or after the noun; in Fijian and many kindred tongues it can only come before. If we can find in those tongues any survivals of the possessive after the noun then it is clear that the Hawaiian usage is the original one.

We do find such a survival in the Lauan dialect: it is preserved in one single idiom *a medha¹ ona*; the cause of it, the reason why.

This is not a living usage, for it is a solitary example, occurring as a set formula. You cannot use *ona* that way whenever you think fit, or find new applications for it: tradition has consecrated this idiom, not the laws of Fijian grammar.

Ona after nouns survives also in Rotuman, though more extensively than in Fijian; it has there come to mean "of" before proper nouns. That is precisely the reason of its survival: it has become equivalent to a preposition, and therefore remains in the position affected by prepositions. *E. g.*

ri on fata: the house of this man, literally "the house of him, this man."

In Tongan the ordinary possessive occupies the same position as in Fijian; but the emphatic possessive comes after the noun.

If we compare the position of one and the same possessive in different languages we are led to the same conclusion as by studying survivals. The *ta*- possessive occurs in Hawaii both before and after the noun, in Wallis island it always comes before, in Eddystone always behind, *e. g.*

W. Is. *ko tana fa'e*: his mother,

Ed. Isl. *na mani tana*: his basket.

Even the suffixed possessive, so called, is not always suffixed. Most Fijian dialects say: *yavangu*: my leg, but some say *nggu yava*. In Eddystone they say, *nggua nene*.

All these facts can only be explained one way: originally the possessive, or more properly the pronoun in the oblique cases had the choice of two positions, but in most Polynesian and Melanesian languages it has been confined to one position only, the other has disappeared, leaving only some survivals.

A FRENCH PARALLEL

It may be asked how such a change may occur. We cannot answer why: we can only insist that such things do happen, are in fact not uncommon. It is not a process conceived solely by the

¹ *th* like English *th* in *the, this*.

imagination without the aid of facts. We need go no further than just cross the Channel to find an exact parallel. Latin enjoyed great freedom of construction; Old French still preserved the option of placing the adjective before or after the noun. Modern French, especially colloquial French, has very little choice left it in this matter.

CULTURE-FUSION AGAIN

If any one still insists on knowing why so many Melanesian and Polynesian dialects should have lost their ancient freedom, we can do no more than suggest one possible explanation, namely culture contact, only not in the manner propounded by Dr. Rivers.

It certainly was not indifferent originally whether the possessive came before or after the noun, just as it is not quite indifferent whether the adjective or the noun comes first in French; some nuance must have been conveyed by the position. Such a nuance would naturally escape invaders who did not possess it, just as most Northern Europeans will miss the fine shade of distinction between "un parfum délicieux" and "un délicieux parfum." Not seeing any difference between the two positions they would drop one and keep only that which seemed more euphonious or which accorded better with their own idiom.

That however is pure speculation; it may be a good working hypothesis; more facts are required to make it into a positive theory. We know so little about the causes of linguistic changes: it is possible that social and economic changes react upon language; the growth or decay of culture must certainly enrich or debase it; the whole subject is as yet so obscure that would-be explanations can never be more than interesting suggestions.

PSYCHOLOGICAL FACTORS

It is fashionable always to give a psychological explanation of customs. Many readers will be disappointed at being offered an historical account instead. There is nothing to be disappointed at; a psychological discourse would have taught us nothing new, if it could have taught us anything at all. It is of course self-evident that mental processes must be involved in the rise and de-

cline of every custom: without mind no custom; but customs exist all over the world and in myriads; if we want to study their relation to mind, modern European customs will serve our turn as well, nay better, than any others. Take survivals, for instance: there would be no survivals if the mind of man was not made the way it is; but if we want to know what is the exact peculiarity that causes them we can investigate it at home more conveniently and thoroughly than in the Pacific. Survivals are as common amongst us as anywhere; if these will not suffice it will avail us nothing to collect more from the Antipodes. Mental processes are involved in the passage from a loose to a rigid order of the sentence; but such developments occur in Europe as well as in the Pacific; they are to outward view exactly similar, so presumably their mental causes are similar. We do not every time a munitions factory is blown up recapitulate all the chemical and physical processes that came into action; these are the same for all; what we want to know is the antecedents. So in philology: we seek for antecedents, we want to know what were the conditions out of which the present state of affairs has developed.

Many will ask what interest can reside in Melanesian possessives if they are not used to throw light on Melanesian mentality. The answer is none at all for the general public. The origin and development of Melanesian possessives interests the specialist only; it interests him as one stone in the edifice he is laboriously building up. The general public is only interested in the edifice as a whole, which is the history of civilization in the Pacific, which is itself but part of a greater whole, the History of Human Culture.

OXFORD, ENGLAND.

FORM AND CONTENT IN TOTEMISM ¹

By A. A. GOLDENWEISER

WHILE the definition of totemism proposed in "Totemism, an Analytical Study" ² must be pronounced as anything but illuminating, it serves to emphasize the two essential elements in totemism which have to a different degree been insisted upon in all theoretical discussions of the subject. For, if totemism may be designated as *a specific socialization of emotional values*, ³ it contains the two basic factors, the emotional values and the specific socialization. The former constitute the content of totemism, the latter the form.

The content of totemic phenomena, that is, the actual beliefs, practices, attitudes involved, were first to arouse the attention of investigators. In McLennan's now obsolete articles on *The Worship of Animals and Plants* totemism is discussed as an aspect of zoölatric and phytolatric phenomena. That the content of totemism is uppermost in the mind of Frazer of the original *Totemism*, appears from his classification of the subject into "clan, sex, and individual totemism." The same applies to Andrew Lang. While the significance, in fact, basic character of exogamy in connection with totemism, was recognized by these authors, exogamy, although in those days often referred to as "the social aspect of totemism," really constitutes but part of the totemic content, thus in no sense corresponding to that specific kind of socialization of cultural features, which in recent years has been discussed as the formal aspect of totemism. No essential departure from the older standpoint is noticeable in the work of van Gennep, or in the systematizations of S. Reinach. Similarly oriented are the studies of Jevons, Gomme, Wundt, and Durkheim, for whom totemism is primarily a

¹ This article is an expansion of a lecture read before the American Anthropological Association, at Philadelphia, in December, 1917.

² *Journal of American Folk-Lore*, vol. XXIII (1910), pp. 179-293.

³ *Ibid.*, p. 275.

stage in the evolution of belief or *Weltanschauung*. Wundt, who deals with exogamy at length, does not seem to have realized the true nature of the formal aspect of totemism. Durkheim, on the contrary, is aware of the fact but fails to develop it or, in fact, to make any effective use of it. It is worth noting that Léon Marillier in his remarkable critique of Jevons¹ reveals his usual insight also in this connection; in fact, his conceptualization of the formal aspect of totemism must be recognized as almost exact. In his latest work on the subject, *Totemism and Exogamy*, Frazer also reaches the insight which, in fact, flows naturally from his extensive familiarity with the descriptive material; but, with the conceptual nonchalance so characteristic of that author, he repeatedly reverts to one of his former positions, apparently unaware of the implied contradiction.

In the analytical study referred to before, the present writer, at the close of a critical survey, arrived at the conclusion that the social aspect was all-important in totemism, in fact, that totemism, or any particular totemic complex, represented a specific socialization of certain religious attitudes. On the other hand, these attitudes, that is, the totemic content, appeared so variable, the religious aspect, in particular, so attenuated, that it seemed impossible to particularize the content in a definition, and hence the concept "emotional values" was introduced for the totemic content. As might, perhaps, have been anticipated, the excessive generality of this definition soon proved to mar its usefulness as a conceptualization of the totemic phenomenon. It was pointed out, with justice, that the specific socialization claimed as characteristic of totemism was equally marked in religious societies,² and again that the term "emotional values" conveyed nothing of the variable but withal sufficiently distinctive content of totemic phenomena.³ Thus a situation arose which threatened to become an *impasse*. At length, however, the writer realized how unreasonable it was to even

¹ "La place du totémisme dans l'évolution religieuse," *Revue de l'Histoire des Religions*, vols. 36 and 37.

² This stricture was first made by Dr. Robert H. Lowie at a seminar after the writer had presented a preliminary outline of his study of totemism.

³ Mr. Sidney Hartland and A. van Gennep were particularly vigorous in their denunciations of this point.

attempt to formulate in a definition the basic principles of a phenomenon as complex as totemism. Thus the resolve was made to substitute for the definition a brief description in general terms, a conceptualized description. This was tentatively carried out in "The Origin of Totemism,"¹ and received its present form in the *Totemism* article of the New International Encyclopaedia, p. 368. To quote:

The set of associated cultural traits invariably present in a totemic community may be designated as a *totemic complex*. What then are the essential constituents of a totemic complex? They are three in number:

1. The totemic tribe is subdivided into a number of social units, usually clans or gentes, but sometimes families or local groups.
2. The people of the tribe possess a set of beliefs and practices—mythological religious, ceremonial, artistic, economic—which almost in all cases center around certain attitudes toward animals, plants or inanimate objects.
3. These beliefs and practices are distributed among the people of the tribe in such a way that the beliefs and practices of each social unit—usually clan or gens—while not identical with, are equivalent to those of all the other social units. The social units are thus constituted equivalent totemic units, while the entire system is a totemic complex.

This summary may be amplified by a quotation from another as yet unpublished paper:

In every totemic community we find the tribe differentiated into a number of social units, clans. Within the limits of such clans the so-called "totemic" features are socialized. The specific content of the features differs from clan to clan, but the form these features assume, their functional relation to the clans, is the same throughout the totemic complex. The totemic complex is thus constituted a firmly knit sociological integer, while the clans appear as equivalent totemic units.

In a recent publication Dr. Franz Boas endorses part of the position expressed in the above statements. That totemism always appears as a tribal complex of social units disparate in content but functionally homologous, is confirmed in the assertion:

Common to totemism in the narrower sense of the term is the view that sections of a tribal unit composed of relatives, or supposed relatives, possess each certain definite customs which differ in content from those of other similar sections of the same tribal unit, but agree with them in form or pattern.²

¹ *American Anthropologist*, vol. 14 (1912), p. 603.

² "The Origin of Totemism," *American Anthropologist*, vol. 18 (1916), p. 321.

The author also condemns unreservedly the views of such writers as Wundt and Durkheim for the excessive emphasis they lay on the "identification of man and animals" involved in totemism. Dr. Boas writes:

It appears to me, therefore, an entirely different problem that is treated by these authors, a problem interesting and important in itself, but one which has little bearing upon the question of totemism as a social institution. Their problem deals with the development of the concepts referring to the relation of man to nature, which is obviously quite distinct from that of the characterization of kinship groups. The only connection between the two problems is that the concepts referring to the relation of man to nature are applied for the purpose of characterizing social, more particularly kinship groups.¹

While two of the writer's positions are thus seen to be supported by Dr. Boas, the last sentence of the above-quoted statement implies an endorsement of the position taken in *Totemism* of 1910, while registering a disagreement with the writer's more recent attitude. For, Dr. Boas's statement condemns all specification of the totemic content, regarding it as significant for totemism, not in its intrinsic character, but only through its association with the social units. At another place Dr. Boas says:

I consider it inadvisable to draw a rigid line between totemic phenomena in a still more limited sense—namely, in so far as the characteristics of tribal exogamic sections deal with the relations of man to animals and plants—but believe that we should study all the customs connectedly, in their weaker form as well as in their most marked totemic forms.²

The variability of content in totemic complexes in different areas leads Dr. Boas to still another conclusion. He writes:

Since the contents of totemism as found in various parts of the world show such important differences, I do not believe that all totemic phenomena can be derived from the same psychological or historical sources. Totemism is an artificial, not a natural unit.³

The two theoretical issues implied in the above quotation having thus come to a head, it seems incumbent upon the writer to advance further arguments in justification of his more recent position.

¹ *Ibid.*, p. 323.

² *Ibid.*, p. 322.

³ *Ibid.*, p. 321.

First of all it seems desirable to disabuse the mind of the reader of any suspicion that the issue involved is one of definition or terminology. Nothing is further from the truth. It is obviously not a matter theoretically indifferent whether the content of totemic phenomena is, so far as the totemic problem is concerned, to be regarded as of no consequence or of specific significance; nor is it theoretically irrelevant whether the unity of totemic phenomena is to be regarded as an abstraction—possibly based on “premature classification”—or whether in that unity there is to be seen a phenomenon of psychological and historical significance. The relevancy of the totemic content and the unity of totemism in different areas are theoretical issues fully on a par with the other two about which agreement is now reached, namely, totemism as an association of historically disparate features into a complex, and totemism as a specific form of socialization.

The propositions to which we now turn are these: the specific content of totemic phenomena, in so far as certain attitudes toward things in nature play so conspicuous a part in them, is not adventitious but significant; totemism, while not presenting in its make-up any new principle not found in other cultural phenomena, is nevertheless a specific institution, deserving as such a separate concept and term.

Let us discuss the second proposition first. That the presence in totemism of certain special attitudes toward nature, is, as such, nothing distinctive, scarcely needs further elucidation; for some form of zoölatry, phytolatry, or nature worship in the widest sense is as universal as the domain of primitive religion. More than that, all the particular aspects of the attitudes to things in nature, such as the recognition of kinship with them, descent from them, community of nature, their appearance as omens, protectors, etc., all of these are also plentifully represented in other non-totemic contents.¹ The social organization with which the totemic content is associated is also a feature not in itself totemic. While the over-

¹ The same, of course, applies to the other elements of the totemic content, religious, æsthetic, economic, etc., all of which in the typical and most common instances appear as correlated with certain attitudes toward nature (*cf.* “Totemism, an Analytical Study,” *Journal of American Folk-Lore*, vol. XXIII, (1910), pp. 251-264).

whelming majority of clan systems appear as carriers of totemic complexes—of this more anon—there are exceptions, which, while rare, are frequent enough not to be negligible.

The particular form of socialization of various features which appears in totemism is also found in different kinds of societies, military, medicinal, religious. In all of these, as in totemism, the different social units—for societies are such, as well as clans, gentes, families, local groups—are differentiated in specific content while being functionally homologous. In many instances it is this feature only which seems analogous to the totemic situation, while the features themselves, the concrete content of the institution is quite different. But in religious societies such as occur, for instance, in the American southwest and northwest, in West Africa, in Melanesia, the content itself is often strikingly similar to that found in totemic complexes.¹ Moreover, all tribal sets of societies, no less than totemic complexes, represent closely knit aggregates of features of historically heterogeneous provenience. Thus, no doubt can remain that whatever principle attaches to the make-up of a totemic complex finds more or less numerous replicas also outside of that content. The purely analytical treatment of the totemic complex, then, serves to deprive it of all individuality in either specific composition, or principle of organization, or historic perspective. It is the writer's opinion that this result points to the limitations of a purely analytical method. A very different light is thrown on the situation with the introduction of the historico-geographical standpoint. As the central feature in this view is the association of the totemic complex with a clan or gentile system, the argument may fitly be opened by a somewhat more careful examination of the theoretical relation between such systems and tribal sets of religious societies. As such an examination has been attempted by the

¹ It may be noted that the classificatory aspect of totemism (*cf.* Boas, "The Origin of Totemism," *American Anthropologist* (1916), p. 326) is also of much wider distribution than that implied in its connection with totemism. We recall such phenomena as college banners and pins, automobile insignia, regimental flags and mascots, street names, and—to mention one most recent instance—the names of British tanks. In all of these instances, including the totemic ones, the psychological as well as logical requirements of the classificatory situation call for the use of a variety of things, names, symbols, which, however, in each instance, remain within the same category.

writer in the Encyclopaedia article referred to before, the passage (pp. 370-371) may be quoted in full:

Totemic communities, as complexes of historically and psychologically heterogeneous features, display certain striking similarities to another form of socio-religious association 'fairly common in primitive groups, namely, religious societies. A religious society is a group of individuals who bear a common name, often derived from an animal, share a set of religious and mythological beliefs, and perform together certain ceremonies.¹ Where the societies occur, there always is more than one society in the tribe, while often a large part of the individuals of the tribe are grouped in religious societies. While male societies are by far the more common, female societies also occur, but almost invariably the membership of a society does not include both sexes, but is restricted to the one or the other. The geographical distribution of religious societies is rather striking. In a large number of totemic areas religious societies also occur, for instance, in the northwestern, southwestern, southeastern and eastern Plains areas of North America, in West Africa and Melanesia. This distribution suggests possibilities of genetic relationship. Webster in his *Secret Societies* has propounded a theory according to which religious societies are to be regarded as totemism in decay, as a normal stage of evolution from totemism to other forms of religious organization. In this dogmatic form the theory must certainly be rejected, but it may contain a germ of truth in so far as genetic relationship between totemism and religious societies may have obtained in individual instances. Thus in the southwest of North America religious societies may have developed out of totemic clans, while in the eastern Plains area, represented by the Omaha and other Siouan tribes, totemic gentes may have grown out of local groups with religio-ceremonial functions. According to recent evidence such relations between the two institutions also seem probable in certain parts of Melanesia. Of even greater interest than the geographical and possible genetic relations between totemism and religious societies, are the similarities and contrasts of the two institutions from a theoretical standpoint. In the one case as in the other the tribe is divided into a set of social units; these units have common functions, ceremonial, religious, artistic; and these functions cluster about or grow out of certain attitudes toward animals, plants or inanimate objects, although the latter feature is by no means as characteristic of religious societies as it is of totemic complexes. In the one case, moreover, as in the other the institution must be regarded as a complex of historically disparate traits. The similarity thus seems almost to approach identity. The contrasts, however, are equally significant. While religious societies, like clans, are social units, they are constituted social units solely by the exercise of common functions. Take away the functions and nothing remains but an aggregate of wholly unrelated individuals. Not so in the case of clans. While it is true that in the case of the clan also its functions determine its precise position in the

¹ Of course, various ceremonial paraphernalia, artistic features, etc., should be added to this statement.

culture of the group, the clan would remain a social unit even if stripped of all its functions. This is due to its social composition, for a clan is a hereditary group of individuals who are in part related by blood and in part assume themselves to be so related. This constitutes perhaps the most fundamental contrast between a clan and a religious society. Other differences are not lacking, however. The religious aspect is almost invariably more pronounced in the societies than it ever is in the totemic clans. The societies are largely uni-sexual, while the clan always embraces related individuals of both sexes. The clan is a hereditary unit, while the society is usually non-hereditary, although certain offices in it may be hereditary, and a tendency towards inheritance of the society itself occurs here and there. Thus what might be called the socio-psychological flavor of a tribal group of societies is quite distinct from that of a totemic complex. Therefore, while the two institutions, somewhat conspicuously coextensive in geographical distribution, present striking similarities in point of cultural content, and suggest from the theoretical standpoint a set of similar problems, it will be profitable to keep them apart conceptually as well as for purposes of intensive study. On the other hand, the comparative study of totemic complexes and religious societies promises to prove a most fascinating aspect of totemic research.

In the light of the above considerations totemism appears as descriptively distinctive, while presenting no special or unique principle in its make-up. The distinctiveness lies in the association of the totemic content with a clan system. If we are to estimate rightly the historical bearing of totemism, as a primitive institution, we must conceive of it as an adjunct, as an all but universal adjunct of clan and gentile organizations. This gives it its specific flavor and accounts for its geographical distribution and, from another angle, for its place in history. For it must be remembered that hereditary kinship systems, clans and gentes, represent one of the two basic forms of primitive social organization, the other being the family-village form, which lacks hereditary social units. Clan and gentile systems have an enormous distribution in primitive culture areas and with it goes an almost equally wide distribution of totemic complexes. If the concept of adhesion is applied here, the two phenomena are seen to appear together almost invariably, the instances where totemic complexes are not based on clan or gentile systems, and those in which the latter are not carriers of totemic complexes being very rare. Thus, the geographical distribution of the two phenomena reveals the fact that there must be here some deep-rooted organic determinant, that there must be some inherent

fitness which draws the contents of totemic complexes into the socializing meshes of clan and gentile systems. To this point we shall presently revert; but before this is done a few words must be said with reference to the alleged artificiality of the concept "totemism."

The concept "totemism" is deemed artificial, not natural, for reasons partly of historical, partly of psycho-sociological order: the historic development of totemic complexes was different, hence they are genetically disparate and non-comparable; the concrete content of totemic complexes is highly variable, hence, from a socio-psychological or cultural standpoint, they are also disparate and non-comparable. The logical limit of this attitude is to regard the concept and term "totemism" as an unjustifiable abstraction based on superficial knowledge of the comparative material or on disregard of significant differences in that material. When these errors are rectified, the concept "totemism" may be expected to become obsolete, its place being taken by a number of less inclusive concepts which would conform more accurately with the concrete data.

To meet this argument the following considerations may be adduced. While it must, of course, be admitted that the specific processes which brought the individual totemic complexes into being must have varied greatly,—in the features that developed, in the order of their development, in the time consumed by the processes of socialization and totemic assimilation, in the hundred and one ways in which in themselves trifling accidental happenings will influence and mould culture,—nevertheless these processes, when viewed in the synthesizing light of historic perspective, reveal certain not unimportant parallelisms.

Thus, the not inconspicuous similarities in the content of totemic complexes must find their developmental counterpart in certain resemblances of the circumstances under which the similar features arose in the different complexes. Again, certain features are obviously more primitive than others, some derivatives of others; and so, wherever the two types of features have appeared in the course of totemic developments, there must have been similarities in the relations of these features. Then again, the very processes

of socialization and psychological assimilation of features, with all the disparity and individuality in special instances, comprise inevitably so many common conditions of a general socio-psychological kind, that the mechanisms at work must have also been similar in many ways.

As to the variability of features in totemic complexes, it is, of course, very considerable.¹ And yet, if the contrasts are set aside, a very respectable nucleus remains which recurs in a large number of instances. Thus, the idea of intimate relationship with the totem, whether in the form of descent, transformation, association, physical or psychic resemblance, or of some other sort; the use of the totem as an eponym; the totem as a symbol, whether in art, or as property mark, or as a sign of rank; these are features of enormously wide distribution in totemic communities; and other features might be named which are only less common. Moreover, in the opinion of the writer, even the following assumption is justifiable. There can be no doubt that the variability of totemic phenomena both in content and genetically speaking is sufficient to discourage any attempt at analogical evolutionary reconstruction in any specific instance. Nevertheless, the similarities referred to above are such as to warrant the expectation that, were the totemic developments all brought to light, a cross-section of their contents could be made at different chronological levels which would reveal an even greater general resemblance than that resulting from a comparison of the complexes now open to investigation.

It appears, then, that from an objective standpoint the contents of totemic complexes and the historic processes which brought them into being may not be regarded as wholly disparate. It is, however, questionable whether, in considering the alleged "artificiality" of the concept "totemism," the objective and genetic standpoints are the proper ones to take.

Many students of culture will admit that the true level for cultural comparisons is the psycho-sociological level, in view of the fact that cultural values themselves lie in that level and that con-

¹ Cf., for instance, "Totemism," etc., *Journal of American Folk-Lore* (1910), pp. 225-228.

stant transvaluations in culture ever tend to play havoc with genetic similarities and differences. While the truth of this is recognized, but very few are willing to concede to comparisons of a psycho-sociological character the same significance which, in their view, attaches to parallels based on genetic relationship. Now, while the latter standpoint is evidently in place when historic reconstruction is the task on hand, cultural interpretations must rely on material which lies in the level of culture itself, the psycho-sociological level—here the genetic retrospect is irrelevant. If this contention is brought to bear on our problem, it presently appears that the real comparability of totemic complexes lies over and above the resemblances in their concrete contents, that this comparability, moreover, is independent of the above resemblances and might indeed persist in their absence. For, however totemic complexes may differ, they all represent totemic cultures to the individuals who are the psychological carriers of such complexes, they represent totemic cultures since they all partake of that specifically socialized supernaturalism which is particularized in the varying clan contents and is synthesized through that spirit of fundamental equivalence rooted in the very nature of a clan system, through the formal identity of the totemic clan functions, through the secondarily derived equivalence flowing from such formal identity of totemic functioning, and, lastly, through the cultural flavor or “feel” of the psychic or cultural level (*Denkart*), which makes that variety of supernaturalism and the particular type of its social transformation congenial to certain societies and cultures. It is considerations such as these that make totemism appear as one of the most characteristic and sharply defined institutions of primitive society, thus vindicating its claim to a separate concept and term.

This brings us to the last point at issue: is it true that the content of totemic phenomena centers, at least in a majority of cases, about certain attitudes toward things in nature? And, if so, must we accept this phenomenon—the adhesion between totemic social structure (form) and a kind of supernaturalism (content)—as a fact, so far unexplainable, but pointing unmistakably toward a deeper connection between the two phenomena? or does not an

analytical examination reveal a certain fitness in the situation which could, at least, form the basis for a future more systematic interpretation? To forestall our conclusion, it is the opinion of the writer that the fact itself of the adhesion is undeniable and that a general theoretical explanation can be offered for its existence. We may dispense with argument in proof of the assertion that certain attitudes toward nature stand, in the majority of totemic communities, in the very center of the totemic content, for most of those familiar with totemic phenomena will not hesitate to endorse the assertion. The question remains: is there any perceivable fitness in the fact as we know it? Can any reason be assigned for the undisputed tendency of certain attitudes toward animals, plants and inanimate things to become associated with the type of social system which underlies all totemic complexes?

The reason, in general terms, seems to be that the social situation in totemism creates certain demands and tendencies which have already been realized in the course of the association of man with nature, hence, they are promptly seized upon and utilized for totemic purposes. To particularize:

In a community subdivided into social units, such as clans, the first demand is for some kind of classifiers, preferably names, which would identify the separate units and yet signify their equivalence by belonging to one category. Again, hereditary kinship groups, such as clans, with a strong feeling of common interest and solidarity tend, so socio-psychological experience shows, to project their community spirit into some concrete thing which henceforth stands for the unity of the group and readily acquires a certain halo of sanctity. It often happens with such objects that certain rules of behavior develop with reference to them, both positive and negative rules, prescriptions and restrictions. Such objects thus become symbols of the social values of the groups. Their very objectivity as well as emotional significance lend themselves readily to artistic elaboration. All along the classificatory aspect remains a fixed requirement, so that whatever traits may develop in the social crucible, appear as homologous traits. Then again, the sense of kinship between members of the individual clans, especially in

view of the absence of precise degrees of relationship and sometimes supported by the genealogical tendency, will often express itself in hypothetical descent from a common ancestor. Also, it would obviously fit the needs of the situation if the above objectivations of the social values consisted of things congenial to man, the properties of which were near and dear to him, of things, however, that would not lie too closely within the realm of specifically human activities, as, in such a case, confusion might result, the sense of property might interfere with the smooth running of the system. Again, it would seem eminently desirable that the things should belong to classes, each one representing a homogeneous group, as this condition would ideally satisfy the requirement that they figure as symbols and objectivations of groups of individuals who, within each group, profess intense feelings of solidarity and homogeneity.

Such, in rough outline, would be the tendencies of a community subdivided into clans.

Now, if the individuals who are the psychic foci of these tendencies had nothing in their experience or psychic content to draw upon to satisfy the demands of the situation, some new creations might be expected to appear which would to some extent satisfy the demands of these social tendencies.¹ But our hypothesis is contrary to fact. For, there exists in all primitive communities a complex of experiences and attitudes which has produced values of just the sort needed in the above social situation, has produced them long before any totemic complex or any clan system have made their

¹ It may not be amiss to note in connection with this interpretation of the totemic complex its differences from as well as similarity to that offered by E. Durkheim in his *Elementary Forms of the Religious Life*. From the standpoint represented above Durkheim's sociology is correct, but his ethnology is at fault. He analyzes the social situation in a way not dissimilar to the one adopted by the present writer. Then he proceeds to derive the totemic complex directly from the tendencies flowing from the social situation. Therein lies the difference of the two positions. It seems to the writer quite unjustifiable and contrary to the economy of effort which is but a correlate of the principle of psychic inertia to assume that new, so-to-say duplicate values will be created in a social situation, in the presence of preëxisting values which, as the above analysis shows, fit admirably the requirements of the case. In justice to Durkheim, however, it must be said that he could not be expected to take this view, as he does not see the way to deriving the psychic values which are here assumed as preëxisting from any other source but the very social situation involved.

appearance among men. That complex comprises the experiences resulting from man's contact with nature and the attitudes flowing therefrom. Among these the experiences with and the attitudes toward animals occupy the foremost place, although those referring to plants and inanimate objects are of almost equal significance. Things in nature have at all times exercised multitudinous functions in human society, and the attitudes they have aroused, matter-of-fact as well as supernatural attitudes, range as far as does man himself. These things, animals in particular, are constantly used for naming purposes, for naming individuals; groups of all varieties, such as families, societies, clubs, game teams, political parties, houses, constellations. They are beautifully adjusted to the function of classifiers, as names or otherwise, for they contain many individuals belonging to the same or to several wide categories, they are familiar and congenial to man, yet lie outside the circle of specifically human things and activities, thus not being subject to the action of those disturbing agencies which abound within that realm. Again, animals, as well as other things in nature, are early drawn into the domain of art, they are painted, tattooed, carved, woven, embroidered, dramatized in dances; they figure in realistic as well as geometric representations, thus also rising into prominence as badges, signs and symbols. Primitive man almost everywhere regards himself as somewhat akin to the animal, and many mythologies abound in animals that were men and in men who are metamorphosed animals. Often descent is traced from animals. Again, it is hard to find a tribe where some sort of prescriptive or proscriptive rules do not exist referring to animals, or also plants or other things. Religious attitudes toward things in nature are as universal as religion itself. Moreover, to the eyes of men organized into mutually disparate and internally homogeneous units, the kingdom of animals and only to a less degree that of plants present a spectacle of strange congeniality: for just as in their own social system, these kingdoms embrace beings or things that belong to the same general kind, but are subdivided into categories that are disparate while internally homogeneous.

Now, it must be remembered that all of these experiences,

relations and attitudes belong to the range of the common human: they are found in most primitive communities and many of them reach far into the historic period including modern life itself.¹ Hence a community organized into definite hereditary social units, say clans, finds itself already in possession of most or all of these experiences and attitudes. But we have seen how in such a community, on account of its sociological make-up, certain tendencies must and, as experience shows, almost invariably do arise. These tendencies point toward just such relations, attitudes, functions, as we have seen have everywhere arisen out of man's experience with nature, particularly with animals and only to a less degree with plants. If these cultural features—for such they are—were not there, the social situation might have created them, or something like them. But they are there. Hence, the demands of the social situation are readily satisfied out of this rich store of pre-existing psychological material. The precise how and when of the process is another story,² nor does it particularly matter. The

¹ This point deserves special emphasis. For, whereas a hereditary clan system or even the tendency toward the formation of one must of necessity be regarded as a relatively late form of social organization, the greatest antiquity must be ascribed to the psychic and cultural phenomena referred to above. The minimum requirement for their origination embraces no more than the psyche of man furnished with the very rudiments of culture, and nature. Therefore we find that those phenomena are omnipresent in primitive society and also extend, in attenuated forms, into the very heart of modern culture, as witnessed to by the ever recurring tendency to anthropomorphize the psychic life of animals, or by ethical vegetarianism, or by the rich store of animalistic metaphor and allusion used in connection with human countenances, characters, affairs, which tend, not always ineffectively, to break down the barriers between man and animal which better knowledge and a matter-of-fact attitude toward life and things have brought into being.

² It will be noted that an attempt is here made to account in theoretical terms for the *content*, or the major part of the content of a totemic complex. Some time ago, in "The Origin of Totemism" (*American Anthropologist*, vol. 14, 1912, pp. 603-605), a similar attempt was made to express in general theoretical terms the *process* which brings a totemic complex into being. This process was shown to be achieved by means of a sociological mechanism for moulding the totemic content into the form of the social system. Certain peculiarities of that mechanism induced the writer to apply the term *pattern theory* to this view of totemic development. The totemic content, in its concrete aspects, received no attention in that theory: the content was treated as adventitious or accidental. In the above discussion use is made of the generally recognized and undeniable fact that the basic part of the content of a totemic complex comprises, in the majority of instances, certain attitudes toward nature. The theoretical analy-

crucial and significant point is this: a group divided into hereditary clans spontaneously develops tendencies the limiting value of which is a totemic complex. For the realization of these tendencies certain psychological or cultural data are required. These are found available. In a situation which, were they absent, might have itself created them, they are utilized promptly and effectively. Thus a totemic complex arises.

It will thus be seen that there exists an inherent and most deep-rooted fitness between the supernaturalism referred to before and the social system which absorbs it. It is, then, to be expected that the vast majority of groups divided into hereditary social units will develop some sort of totemic complexes. And such is found to be the case.

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sis made above represents an attempt to provide a general explanation of the association of that specific content with the totemic social system. The crux of the explanation lies in the discovery of a marked correspondence between the tendencies arising in a social system of the type involved and the ideas and attitudes springing from man's contact with nature. This correspondence or fitness leads to the merging of the above ideas and attitudes with the social system. Hence this theory of the totemic content may be designated as the *fitness theory*. In it the form is not represented as creating the content, as is the case in Durkheim's theory; but the social form is shown to develop certain tendencies which have some such content as their limiting value, hence the content, here shown to be preëxisting, is absorbed by the social form.

A PORTO RICAN BURIAL CAVE

By ROBERT T. AITKEN

DURING the months of June and July, 1915, the field work, of which this paper is the report, was performed by Dr. J. A. Mason, now of the Field Museum of Natural History, and the writer, under the direction of Dr. Franz Boas.¹ The portion of the work here described includes the excavation of a cave and of a so-called *juego de bola*. The cave in question was first visited by Dr. Mason earlier in the year when he happened to be in the neighborhood in connection with his work of collecting folklore. The cave is known to the natives simply as Antonio's *Cueva*, or as the *Cerro hueco*, and has for years been used as a place for drying beans and corn. It was known to have yielded human bones, as a certain citizen of Ponce, P. R., is reported to have removed the skeleton of a child at some indefinite time in the past. Dr. Mason, at the time of his first visit, merely scratched the surface at one or two places inside the cave, but even with such a superficial examination found a few scattered human bones. When, later in the year, opportunity was afforded for more extended archaeological work, this cave was selected as the first to be investigated because of the favorable impression gained from Dr. Mason's report of his first visit.

¹ The anthropological division of the Natural History Survey of Porto Rico, conducted jointly by the New York Academy of Sciences and the Porto Rican government, is under the direction of Dr. Boas, and was in 1915 conducted in several sections. The report of one section, by Dr. Herman K. Haeberlin, has already appeared. ("Some Archaeological Work in Porto Rico," *American Anthropologist* (N. S.), vol. 19, no. 2.)

I wish here to express our indebtedness to Mr. Leopold B. Strube, of Utuado, Porto Rico, for his willing and able assistance in dealings with the country people, and in acting as agent for the party in Utuado throughout the summer. We are grateful also to Dr. Charles P. Berkey, of Columbia University, and to the late Mr. Gratacap, of the American Museum of Natural History, for furnishing the mineralogical and geological information used in this paper. The soil analyses and the information concerning the texture of the pottery were also supplied by Dr. Berkey.

The actual time spent in the excavation of the cave was about two weeks, the party during this time being camped in the mouth of the cave so as to lose no time in useless traveling. All the earth removed was thrown down the steep hillside, so that when the work was completed, the cave was entirely stripped of earth down to the calcite layer found to mark the limit of the specimen-bearing earth. All the material collected is at present at the American Museum of Natural History in New York.

THE CAVE

Antonio's *Cueva* is located in a limestone ridge in the *Barrio Caguana*, about ten miles west of the town of Utuado. The island of Porto Rico is made up of two different geological formations, volcanic and limestone. The dividing line between these two formations in the neighborhood of the cave is the Tanama river, distant about two miles in a direction almost due west, the course of the river at this point being nearly north-south. The hills and ridges of the limestone formation are everywhere fairly honeycombed with caves,¹ varying in size from the merest rock shelter to caverns of vast extent. Many of these caves are practically inaccessible, or at least are to be explored only with the aid of ropes, ladders, and artificial light. Some are rendered impassible by the presence in them of small or large bodies of water, or by their forming a portion of the course of creeks or rivers. Fortunately for the purposes of the expedition, this cave suffers from no such disadvantage. It is located about two hundred feet above the floor level of a small bowl-shaped valley, enters the hill in a direction nearly horizontal, and is perfectly dry. Moreover, the entrance is large and faces east, thus making the use of artificial light unnecessary. The comparative ease of access from the nearby roads and trails, and the presence in the neighborhood of an abundance of cheap labor, in combination with the above mentioned conditions, made the work of excavating the cave fairly simple.

At the present time the cave extends entirely through the

¹ Fewkes, "The Aborigines of Porto Rico and Neighboring Islands," *Twenty-fifth Annual Report Bureau of American Ethnology*, p. 87.

hill in an east-west direction, the extreme length being about one hundred twenty feet. A partial barrier of stalactites and stalagmites about sixty feet from the entrance divides the cave into two main chambers, of which the western is somewhat the larger. The western entrance seems to be of comparatively recent origin, having evidently been formed by the collapse of the roof at the western end of what until that time must have been a dark, damp, inside cavern. This supposition is sustained by the appearance of the fallen section of the roof, also by the fact that our investigation yielded not the slightest trace of human occupation or use of any sort of the western chamber.

The eastern entrance is about sixty feet wide, and is divided by a central pillar about twelve feet in diameter. The ceiling is about twenty-seven feet above the floor at this point. The sketches in figure 25 will give an idea of the size and relative positions of the different portions of the cave, and also of the stratification of the floor. Chamber *B* is only indicated, as excavations in it were fruitless. Chamber *A*, which yielded all the specimens found, is about sixty feet in width and length, the ceiling sloping up slightly from the entrance to a central point, about thirty feet above the floor, then rounding down to the walls, forming a rude dome. An upper gallery, marked *C* in figure 25, extends about forty feet in a westerly direction, terminating in a chimney-like aperture opening upon the precipitous western side of the hill. The gallery opens into the southwest wall at a height of about eighteen feet above the floor level. There were numerous niches and crevices about the walls, formed by the characteristic stalactites and stalagmites. The only one of any size is indicated in the sketch (fig. 25) at *D*, in the south wall.

The floor of chamber *A* slopes from the entrance down about one foot in six. The southern half is a terrace, approximately level, which terminates at the center of the cave, and drops abruptly to the level of the remainder of the floor. From this central line there is a slight upward slope to the north wall, forming a central trough, the south side of which is considerably higher than the north. In the survey of the cave accurate measuring devices were not

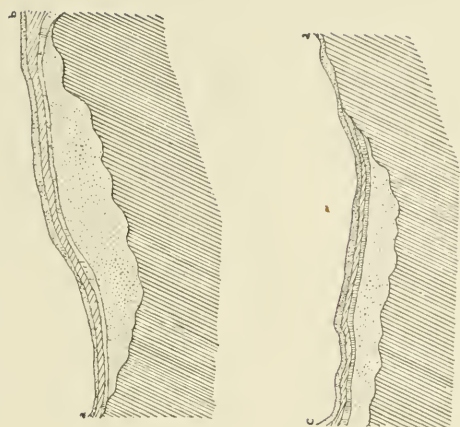


FIG. 25.—Groundplan and Cross sections of Antonio's Cueva.

available, and the bearings were taken with a small pocket compass, not corrected for deflection, so that distances and directions are only fairly close.

The recesses of the walls were first examined for possible hidden objects. But if there ever were any such, other hands than ours must have removed them long since, as not even a bit of potsherd rewarded our search. No paintings or rock-carvings of any sort were found on the walls, and the dryness of the cave at the present time makes it seem unlikely that there may be any such covered over by the drip from walls or ceiling. This dripping in the outer chamber (*A*) is so slight as to be negligible.

After this thorough search of the recesses, and a careful examination of the surface debris, the real excavation was begun. The method used was quite simple, and consisted in removing the floor a layer at a time. No trenching was done. The tools used were the long blades known as *machetes*, the universal tools of the Porto Ricans. For breaking through the calcite layers a pickax was necessary. For the actual uncovering of the specimens a small trowel was used, and much of the earth in these cases was removed with only the fingers as tools. This process was necessarily slow, but it was justified by results, as the remains found were so exceedingly fragile that only by this stripping off of the superposed material could they have been removed intact.

Work was begun simultaneously in two different places at the western end of Chamber *A*, on the terrace and at the barrier separating the two main chambers. The excavated earth, after careful examination for small objects, was carried to the mouth of the cave and dumped down the hill. In this manner the surface layer covering the entire floor was removed, without discoveries of importance. The only objects found were a few long exposed and well dried human bones, scattered through the litter, showing no evidence in their position of any attempt at interment; a few snail shells of different varieties, and some bat skeletons.

Scattered over the floor of the cave were sections of fallen stalactites, ranging in size from small bits to pieces weighing a hundred pounds or more. These sections were more or less im-

bedded in the surface layer, and were removed with it. When one of the larger of these was removed, it was found that the earth beneath it differed from the surrounding soil at the same depth. It was uniform with the surface soil, and not hard packed as was the soil at this depth elsewhere. Investigation showed that it covered human bones, which proved to be a complete human skeleton. This skeleton lay at a depth of about twenty inches from the original surface of the floor. In its immediate neighborhood the strata were undisturbed, and consisted of the surface soil and two successive layers of hard-packed, red-brown earth, separated by a layer of crystalline calcite two inches thick. A second calcite stratum lay below the lower layer of earth. This second calcite layer had been just broken through, the skeleton lying partly below, partly above. There was no stratification immediately above, the overlying earth being homogeneous to the surface. An examination of the upper calcite layer showed that an opening had been broken through it just large enough to allow the body to be placed in its final position.

The skeleton lay in the familiar contracted position, on the right side, facing north. It evidently had not been disturbed after interment, as all the bones were in their natural relative positions. All the major bones were recovered, a few of the small bones of the hands and feet being the only ones that were not found. All were in comparatively poor condition, being badly crumbled. The skull, however, was removed intact, the mandible with it. The skull has at this writing not been measured, but was apparently not artificially deformed, and was noticeably brachycephalic. The skeleton was that of a young adult. All the bones were after removal exposed to the air to dry and harden, then later packed in native cotton for shipment to the Museum in New York.

The above-mentioned burial was the first of twenty to be uncovered within the cave. The majority were of young adults or of children. The material has as yet not been arranged or measured, so little can be said at present regarding it. All of these burials were found at a depth of from fifteen to twenty inches, and in no case did the earth above show stratification. In some cases

the earth had been removed to the first calcite layer only in making the burial, while in others, as in the first, the calcite had been broken through, and the burial made at a lower level. The first burial was found at a point nearly in the center of Chamber A, at the bottom of the trough-like depression. This probably explains the poor condition of the remains as compared with the fairly good condition of many of those found at other points. Most of the water seeping into the cave or driven in in the course of the frequent rainstorms must have found its way down this central trough; consequently the soil underlying it was moist, and the remains only poorly preserved. All the remaining burials were along the north wall, some so near as to be almost touching, others, the last two found, lying in the center of the northern half of the cave entrance. There were two sets of exceptions to this rule. Four of the burials (collection numbers 4, 8, 12, 14) had been disturbed apparently by subsequent burials, so that it was difficult or impossible to tell exactly what the original position of the remains had been. Four others (17, 18, 19, 20), all of children, were lying in a north-south position, contracted, with the head at the south, facing east.

Two of the burials that had been disturbed (8 and 12) contained the bones of a child or infant together with the bones of an adult. No attempt was made at the time of excavation to ascertain sex, as the bones were in such a badly decayed condition that handling was dangerous. One burial (14) that had been disturbed lacked the skull and both femurs. In all the cases where the burials had been disturbed there were bones in excess of those belonging to the principal skeleton, suggesting that scant respect had been paid to earlier burials when they were encountered in digging new graves. Three of the burials yielded complete, intact skulls, mandibles included. It is possible that some of the other skulls are sufficiently complete to be reconstructed. The majority, however, are beyond repair. This was in several instances due to the fact that large blocks of calcite were frequently placed directly over the burials. The weight of these served in time to crush the more fragile bones.

After the last of these skeletons had been removed, the little remaining earth was excavated down to the clay, but no more remains were found. No remains of any kind were found in this clay, although this was excavated in one place to a depth of over five feet, at which depth the irregular blocks of calcite of what seemed to be the final rock floor of the cave were exposed. The upper gallery was carefully excavated, as were all the hollows and recesses of the walls, but without result. A large recess in the southern wall (*D* in figure 25), from which the child's skeleton already referred to was reported to have been taken, was excavated down to the solid limestone of the hill. A single human rib was found in the break in the calcite from which this skeleton is supposed to have been removed, and since it was the rib of a small child it lent color to the story.

In addition to the human remains above described, there were a few other objects found that may be important. Several portions of skulls, also several of the long bones of a strange variety of rodent were found in different layers, being apparently coeval with the human remains. Subsequent examination of these bones has shown the rodent to be of a new genus and species. These bones belong to the same animal as is described by Dr. J. A. Allen, of the American Museum of Natural History.¹ The remarks by Dr. Haeberlin, in his Porto Rico report² concerning the rodent bones found by him, which were the ones examined by Dr. Allen, apply likewise to those found in Antonio's *Cueva*.

There was a complete absence of all mortuary offerings. Nothing whatsoever seems to have been buried with or placed near the body. The graves were not marked in any way, unless the large blocks of calcite placed above some of them were intended as memorials. Possible exceptions to the first statement above may have been certain small flat bits of crystalline calcite, of which a dozen or more were found scattered through the upper layers of the floor. They were, as a rule, not associated with the burials, only one having been found near skeletal remains. Even in this

¹ *Annals of the New York Academy of Sciences*, vol. XXVII, pp. 17-32.

² Haeberlin, *American Anthropologist* (N. S.), vol. 19, no. 2, pp. 225-227.

case the association seemed accidental, as it lay several inches above the bones. These calcite bits were each from one to two inches square, and from a quarter to three-eighths of an inch thick. Many had been pierced, which suggests the possibility that they may have been worn as ornaments. Of these Dr. Berkey writes:

Specimens 25.0/1404 are crystals of calcite. They have been bored through, as shown by the straight hole, its uniform diameter (or slightly larger towards the ends), the independence of crystal axes and cleavage, and the crushed surface of the hole. The material may be wave-rolled fragments of a calcite vein in sea-eroded tuff.

In different places, and not associated with the burials, two fragments of stone were found, the shape of which led us strongly to suspect human workmanship. Dr. Fewkes declared that at the time of his investigation, 1904, there had been no mention made of the finding of any chipped or flaked implements in Porto Rico.¹ Yet a microscopic examination of these bits shows not only flaking, but a distinct secondary chipping along the cutting edge. These implements, if such they are to be considered, are quite small, being approximately five cm. long by one and a quarter cm. wide. These also were examined by Dr. Berkey, who says of them:

25.0/1403 is chert material (impure flint), microcrystalline silicon dioxide with some hydrated iron oxide present. They are artificially shaped. The whitish material on the angular face of the smaller piece is the original surface. The sharpness and freshness of the edges, which have not been worn since made, indicate a chipping or cracking (under influence of first heat, and then water) origin. The larger specimen shows this extra well. They are not possible erosion or weathering fragments.

Potsherds were conspicuous by their almost complete absence. Only a very few, about a dozen in all, small fragments were found, and these widely scattered. No complete pots were found, and the pieces were so fragmentary and so few that they cannot be assembled into a form sufficiently complete to show the shape of the whole. The photomicrographs (fig. 26), for which we thank Dr. Berkey, are of the two types of pottery found here. The following information concerning the two specimens was also furnished by Dr. Berkey.

¹ Fewkes, *Twenty-fifth Annual Report, Bureau of American Ethnology*, p. 91.

The field (25.0/1406) shows a complex mineral make-up, chiefly of angular grains of various sizes and composition, bound together in a comparatively small amount of matrix of darker (brown) substance. The larger and lighter-colored grains are chiefly fragments of feldspar and this is the chief identifiable constituent of the mixture. The dark grains probably have ferro-magnesium content which has been somewhat affected (turned brown) in the burning or aging of the ware. Occasional grains are practically black and represent still higher iron content. Rarely in other portions of the thin section fragments of rock rather than individual minerals may be seen; but in this ware such occurrences are evidently unusual. In certain parts of the specimen a slight streakiness or flowage structure may be seen, but this also is obscured and variable. The large amount of feldspathic content, the large proportion of mineral fragments of this sort, and the comparatively small proportion of matrix matter, together with the brown color of the matrix, are the microscopic characteristics of this particular specimen.

The following information relates to specimen 25.0/1425, figure 26.

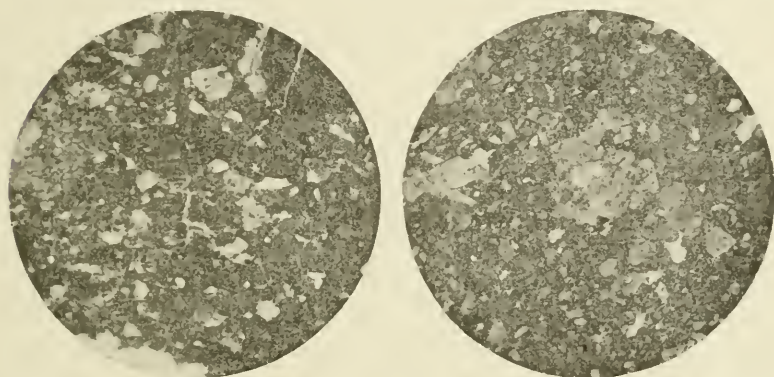


FIG. 26.—Photomicrographs of pottery from the cave; left 25.0/1425, right 25.0/1406.

This particular specimen (25.0/1425) contains very variable fragments, some of which are feldspathic mineral fragments; others are quartz, and still others are fragments of rock with aggregate composition. The binding matrix, representing the original clayey matter, is in comparatively large amount; and the proportion of mineral fragments is not nearly as large as in 25.0/1406. There is also much greater variety of quality of fragments in this specimen than in the other, and a greater variation in the condition of the material, that is, the matrix is much browner in some parts than in others of the field. There is no appreciable streakiness or flowage structure noted in this specimen. This is apparently a much less carefully selected mixture of material than 25.0/1406, or else it represents the manufacture of a locality where the more specialized quality of material was not available.

The sherds found showed little or no ornamentation. What little decoration there was was of the type described by Dr. Haeberlin in his report of the excavation of the *Cueva de la Seiba*,¹ that is, consisted of simple incised lines, with no scroll work or curvilinear design. A comparison of these specimens with those described by Dr. Haeberlin seems to show a relation between the pottery found in the *Cueva de la Seiba* (fig. 18, p. 228) and 25.0/1425, above, and between that found at the *juego* (fig. 19, p. 228) and 25.0/1406, above. Points common to the first group are: greater variety of size of grain; larger amount of clayey matter or matrix; greater variety of composition of grain. Points common to the second group are: comparatively small proportion of matrix material; great abundance of clear mineral matters; the apparently careful selection of materials. A comparison of the photomicrographs in figure 26, above, and figures 18 and 19, p. 228, of Dr. Haeberlin's report, will bring out clearly the resemblances between the two sets of specimens.

Reference has already been made to the stratification of the floor of the cave. Samples were taken from various places at different levels, which have since been examined by Dr. Berkey.

The top layer, after the surface litter of vegetable matter and guano was cleared away, was found to cover fairly uniformly the whole of Chamber A. Near the entrance this layer was high in plant remains, and contained mostly leaf fiber remnants, with a little charcoal. It was, however, highly calcareous. Farther in this organic material became less, practically disappearing a few yards from the entrance. The thickness of this top layer varied from almost nothing to eight or ten inches, and covered many of the smaller fallen pieces of stalactite.

Below the top layer came a series of alternate layers of hard packed earth and crystalline calcite or stalagma. These latter plates were in most places easily broken through with a pick, but in several places, especially near the walls, they were as much as five or six inches thick. The layers of earth varied in color, the darkest being a deep brown, the lightest an ash gray. Analyses

¹ Haeberlin, *American Anthropologist* (N. S.), vol. 19, no. 2, p. 228.

of samples from various of these layers show them to be of essentially similar composition, differing mainly in the proportion of their constituent elements. The average composition was lime powder, about 80 per cent., calcite crystals, about 10 per cent., the remainder varying percentages of charcoal fragments, gastropod shells, broken bits of crab shell, and grains of quartz. Some of the lime powder showed stains of limonite. The deeper layers contained a higher percentage of clay, and below the third, or in places the fourth, plate of stalagma, the soil was entirely clay. At one point this was excavated until over five feet had been penetrated, the solid rock floor of the cave being encountered at this depth.

The small deposits of charcoal and wood ash found at many points throughout the cave were evidently at least in part due to the charring and burning of the roots of trees and vines growing outside the cave. Brush fires were said to be of rather frequent occurrence, and traces were found in the cave of roots, partially burned, partially rotted. There were no stones showing the action of fire, and none of the ash deposits were large enough to suggest hearths.

JUEGO DE BOLA

After completely excavating the cave, attention was turned to the valley at the foot of the ridge in which the cave occurs. The cave is at an elevation of perhaps two hundred feet above the floor level of the valley, the surrounding hill tops being about as much higher again. The valley is bowl shaped, and has an area of not over one square mile. Directly below the cave are the remains of a *juego de bola*, or ball court, this term being applied generally in Porto Rico to the remains of prehistoric villages and settlements of all sorts.

The *juego* in question is quite small, occupying an area about three hundred feet square. It consisted of a main terrace, indicated at *A* in the sketch (fig. 27), with fairly definite boundaries, marked by a roughly semicircular wall or row of stones, surrounded on three sides by other smaller terraces, similarly partly bounded by irregular walls.

There were two small mounds, one near the main terrace, the other at the opposite end of the valley. The former was only

a small heap of earth, in which were found only a few bits of potsherd, and a few pebbles, the entire heap being only about two feet high and four in its longest diameter. The potsherds were similar to the two types found in the cave, described above. The second mound was less impressive than the first in its content, as it yielded

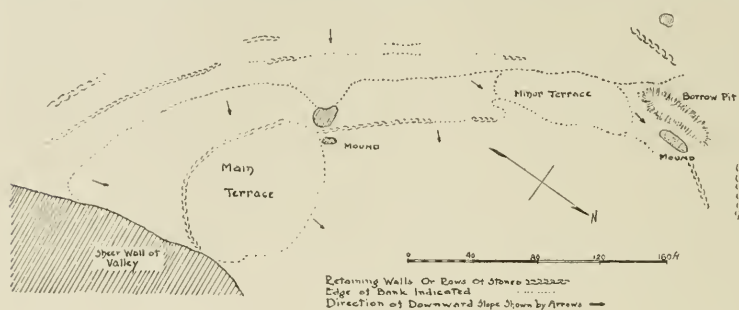


FIG. 27.—Groundplan of Jugo de Bola.

nothing whatever except earth exactly like the surface soil surrounding it. The dimensions of this mound were larger, it being about twenty by ten feet, and about four feet in height. There were a very few potsherds found scattered in various sheltered crannies near rocks on the different terraces. All were of the types already described. A few more pebbles were also found. These were of igneous rock, and must have been brought from a watercourse at some distance, as the nearest volcanic formation is at the Tanama river, two miles away.

There were numerous rows of stones heaped up with more or less regularity about the floor of the valley. Some of these were sufficiently well laid to form walls, the terraces mentioned being bounded on the lower sides by such retaining walls. Excavation behind these retaining walls showed conclusively that they were artificial, and that they had been in place a great many years. We were unable to thoroughly excavate the site, as it was covered with coffee shrubs, and the owner was averse to having any of these damaged. Just enough excavating was done to prove the walls artificial and probably prehistoric, and to examine the two mounds mentioned above.

The general disposition of the walls, terraces, and minor rows

of stones in relation to the main terrace led us to suspect that here was the site of a small village, with perhaps a dozen or more dwellings, each of the dwellings being located on its own little terrace, all grouped around the central terrace or court. This is of course mere speculation, as the excavation was too superficial to warrant any conclusions beyond that here was some sort of a settlement.

Aside from the potsherds and pebbles, the only object of prehistoric origin found was a hammerstone (fig. 28), picked up by Dr. Boas near the *juego*. This was the only implement of its sort found by us here or elsewhere. It is a rough pebble, 11.8 cm. in length by about 9 cm. in diameter, grooved about the center. The head was worn as from hard use. Mr. Gratacap thought it quartz porphyry (volcanic).

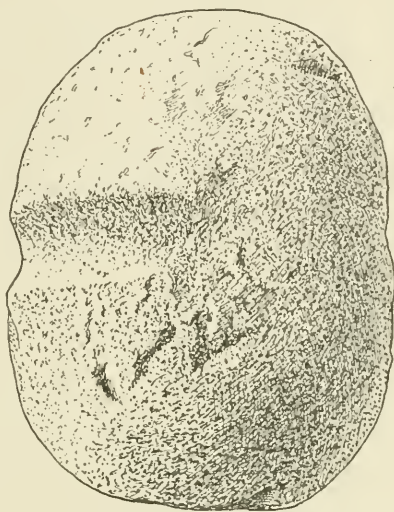


FIG. 28.—Hammerstone (25.0/1411) from Juego de Bola.

While working at the cave we were continually being informed of other caves where skeletal material was to be found, and on one occasion a fairly complete human skeleton was brought to us. No details could be learned as to its position and surroundings when found, beyond that it was but one of many to be found at that particular cave. A string of beads, said to have been associated with the remains, was also brought. These latter looked suspiciously modern. Time did not permit of our personal examination of the cave in question. Later we noted that a cave was located not far from a village site excavated by us, and the report was that skeletal material was to be found in this cave also. There is no question but that further investigation would yield remains well worth the excavation.

HAND SIGN OR AVANYU

A NOTE ON A PAJARITAN BISCUIT-WARE MOTIF

BY LUCY L. W. WILSON

THE word "avanyu" has been given to a decoration motif frequently found on prehistoric Pajaritan biscuit ware.¹ It is a motif that still survives in some of the Rio Grande pueblos, notably San Ildefonso, and also in the Tewa village at Hopi, although much modified. So far as I know, this design has not been found on any prehistoric pottery except that from the Pajarito. It was, apparently, much more popular at Otowi than in the other towns of this site.

Although conventionalized, scarcely two drawings of the symbol are alike. The variations are so numerous and so marked that it is easy to make an extensive classification of them.²

Whatever may be the meaning of the symbol, it is certainly not an avanyu. Avanyu is the Tewa word for their mythological plumed serpent. Doubtless, the zigzag form of the god is due to the appearance of lightning, which in this country of thunder-



FIG. 29.—Drawing of an Avanyu made by a San Ildefonso Indian.

storms so frequently precedes the coming of the much desired rain. On the walls of the kivas of San Ildefonso and Jemez, he is thus represented as in figure 29.

¹ Hewett, E. L., *Communautés Anciennes dans le Désert Américain* (Geneva, 1908), pp. 91-2; Kidder, A. V., Pottery of the Pajarito Plateau and of some Adjacent Regions in New Mexico, *Memoirs of the Anthropological Association*, vol. II, pt. 6, pp. 431-34.

² Kidder, *op. cit.*, p. 432.



PETROGLYPH AT TCHERGE OF THE AVANYU OR PLUMED
SERPENT



PRAYER MEAL BOWL FROM OTOWI

Crude petroglyphs of the avanyu, the plumed serpent, are frequently seen on the walls and in the caves of the Pajarito. Probably, many of these representations are modern, too. But the finest of all, at Tcherege, on the old Ramon Vigil grant, is undoubtedly prehistoric (pl. 1, fig. 1). So, too, are drawings on excavated pottery, such as that on a small prayer meal bowl found at Otowi (pl. 1, fig. 2).

It is excusable, perhaps, to suspect an avanyu whenever the decoration is markedly zigzag. But it is farfetched to think that a design whose occasional zigzaggedness is obviously an adaptation to the shape of the article decorated must therefore be an avanyu (pl. 1, fig. 2).

That this motif was much more probably a hand sign, was suggested to me by the following facts:

First, unmistakable hand signs occur on the rocks of Otowi. On the perpendicular wall of the high mesa which bounds Otowi

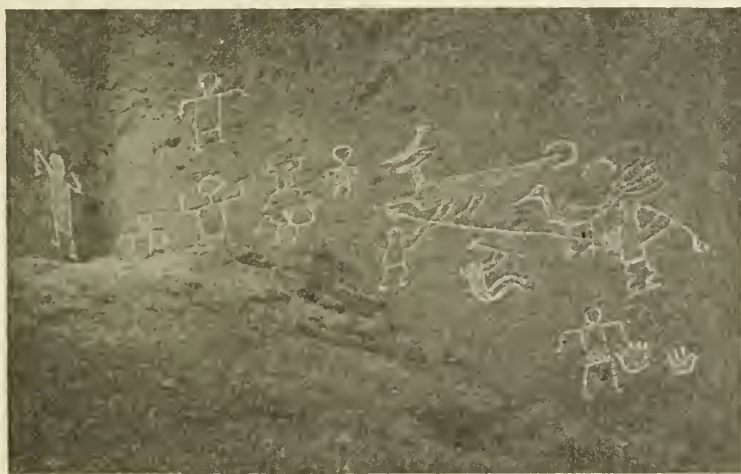


FIG. 30.—Petroglyph from Otowi; battle scene.

on the north, just above a two-storied porch-front cave dwelling, is one of the most interesting petroglyphs of the region. Apparently it represents a battle scene. Perhaps, the deer, the two birds, the sun, the moon, the arrow, indicate the clans that took part in the

fight. But what mean the two hand signs to the right? fig. 30.

Second, is the universality of the hand sign across the ages and throughout the world. The hand sign has been a favorite symbol both in prehistoric and in primitive art. In several of the French caves there are impressions of hands, apparently made by blowing a black or red powder on a sticky impression.¹ Red hands are more or less frequently met with in Mayan art.² In northern India, one sometimes sees on whitewashed walls what looks like impression of a human hand, reddish-brown in hue. Among the Madigas, when a marriage takes place, the slaughterer of the sacrificial animal places his blood-stained hand against the wall.³ In the Ghetto of Jerusalem, a large whitewashed "Hand of Might" is placed over the door of dwellings to protect the people within from misfortune and death.⁴ Among the Dakota, the Winnebago, and certain northwestern Algonkian Indians, it was a common practice to press a hand previously smeared with white or colored clay on the naked breast or shoulder or some other part of the body in order to prepare a dancer for the ceremony.

The figure of the human hand is used by the North American Indians to denote supplication to the Deity or Great Spirit; and it stands in the system of picture writing as a symbol for strength, power, or mastery thus derived. . . . The design of the hand . . . used disjunctively . . . is the most mysterious . . . precisely because there are no accessories to help out the meaning.⁵

The use of the hand sign by primitive people is apparently either to call the attention of the gods to a vow or prayer, or else to avert the evil eye. Perhaps, however, these are secondary functions, and the hand actually represents Power, a god in high repute, even in these days. Certainly, this is and was its meaning in representations of the numerous many-armed gods and goddesses of oriental mythology.

Of course, we shall never know the meaning of this symbol to the ancient Otowians, nor even whether it really was a hand sign. The

¹ Elliot, G. F. Scott, *Pre-historic Man and His Story* (Philadelphia, 1915), p. 305.

² Stephens, John L., *Incidents of Travel in Yucatan*, vol. 2, p. 46.

³ Thurston, Man, quoted by Elliot, *op. cit.*, p. 306.

⁴ Lees, *Village Life in Palestine*, p. 215.

⁵ Stephens, *Travels in Yucatan*, vol. 2, appendix, p. 476, *et seq.*

most cursory examination of the subjoined drawings figs. 31 and 32, traced from pottery excavated at Otowi, will convince anyone that whatever the figure may be, it is certainly not an avanyu.

No other detailed study of the material excavated from Otowi during the summers of 1915-16-17 has yet been made. Nevertheless, the following general account of the expedition, with special emphasis on the "high lights," may have some value to others working in the same field.

Otowi is in the Pajarito Park part of the Bandelier National Monument, about twenty miles northwest from Santa Fe. With its companion sites, Tsankawi, Naviwi, Tscherege, it was deserted before the coming of the Spaniards. The first historic mention of these ancient towns was in Bandelier's Second Report, but he never visited them, however.

Fifteen or twenty years ago, Dr. Edgar L. Hewett, now director of the Museum of New Mexico, Santa Fe, together with two Indians, excavated the burial mound of Otowi. The pieces of pottery came out just as fast as he could take care of them. The very considerable result of his two weeks work is now installed in the Hall of the Southwest in the National Museum, Washington. There are indications that, from time to time, holes were dug, later, by private individuals, hoping,—perhaps, succeeding,—in finding pots. Practically, however, the entire site, exclusive of the burial mounds, has been excavated, under my direction, for the Commercial Museums, Philadelphia, and all of the material thus secured is now on exhibition there.

There are three large community houses on the mesa of Great Otowi. South House is the oldest of these, to judge from its poorer masonry, comparative absence of communicating doors, lower walls, and the harder earth filling contained in its seventy-six rooms. East House is smaller, consisting of only forty-five ground-floor rooms, while North House, a great E-shaped pueblo, contains two hundred and seventy ground-floor rooms. The comparative absence of doors, the relatively large amount of adobe in the weak walls, and the comparatively small amount of stone on or near the surface of East and South House, all incline me to believe that these

houses were only a single story high. North House, judged by the same standards, had a terraced second story, and possibly, not probably, a few third-story rooms. All of the rooms of all of these houses were excavated to bed rock. There was no evidence of secondary occupation although there were many burials in what were evidently abandoned rooms. Doubtless, all three houses

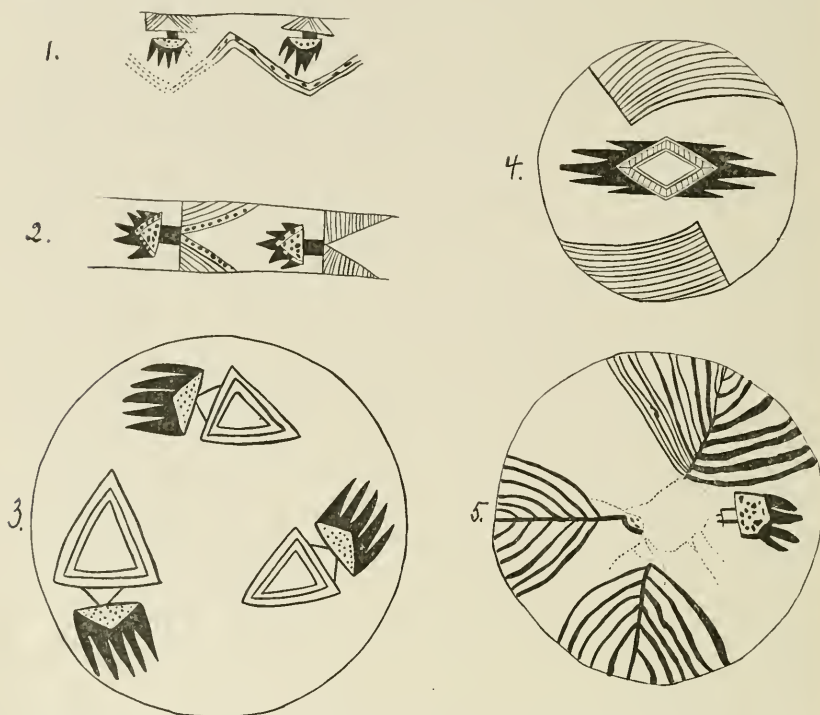


FIG. 31.—Hand Type: 1 and 2 from bowl exteriors; 3, 4, and 5 bowl interiors.

were occupied contemporaneously, although the inhabitants may have been slower in deserting the newer and larger house.

On a low ridge, a few hundred feet south of Great Otowi, there are two groups of community houses apparently contemporaneous with South and East Houses on the larger mesa. One of these groups consists of two buildings, a rectangular house of forty rooms, all excavated, and the other a hollow square of perhaps a hundred rooms. The other large house is another hollow square of about the

same size. On this same ridge, there are also at least seventeen "small house" ruins. On a still lower ridge, parallel but farther south, there are three other small-house remains. On the mesa of Great Otowi itself there are the unmistakable indications of at least seven such small-house pueblos, three of them with kivas.

In brief, within half a mile of each other, in the narrow canyon

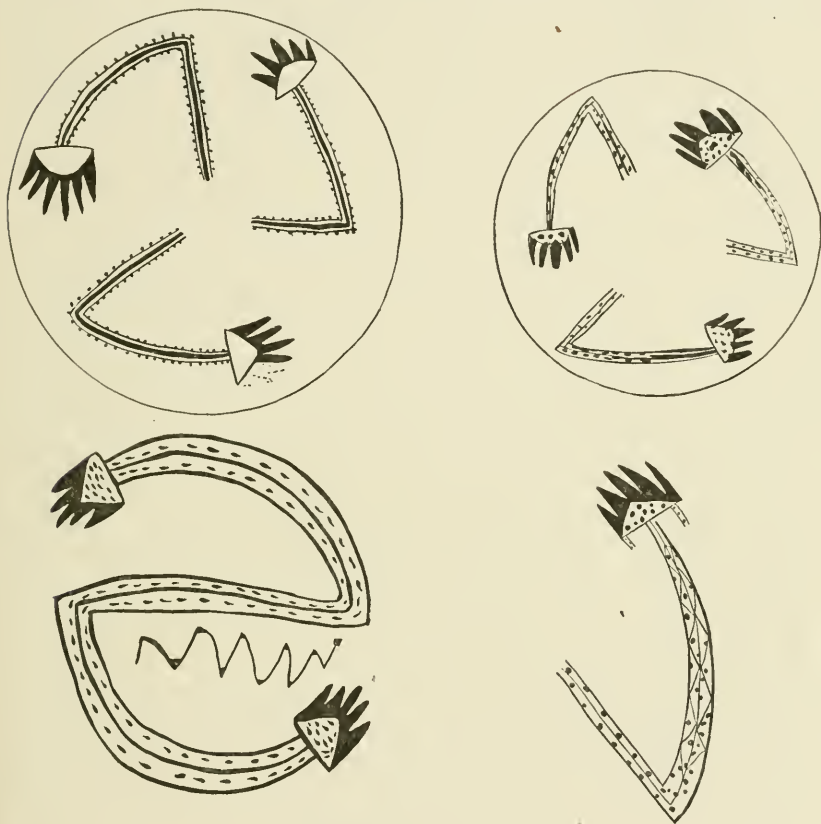


FIG. 32.—Hand Arm Type.

of Otowi, there are indications of at least twenty-six farm houses belonging to the period before the "concentration," and of six large houses of the later period. Moreover, there are many groups of at least two-story porch houses in the almost perpendicular walls that bound the canyon on the north. These cliff dwellings were occupied

contemporaneously with the large houses, and may, indeed, have been the winter homes. Still, it is difficult to believe that the population of this little valley could have been much less than a thousand in the period just before the great concentration, or less than two thousand in the palmiest days afterwards. Probably the depopulation was due to a gradual migration to the better watered and more fertile lands of the Rio Grande. Nevertheless, a protective stone wall and the location of a reservoir so that it could not fail to receive drainage from the large burial mound do suggest that there might have been cataclysms due to enemy attacks and to pestilence that hastened the desertion of Otowi.

Scarcely a day passed in our three summers of work that we did not uncover something of interest. Still, certain events stand out in red letters. The first of these was the uncovering of the body of a man, buried, not in a room but in a passage way between the outside wall of North House and a heavy stone wall without. There were the remains of a covering cloth, a large basket, three long notched rain sticks, half a dozen prayer sticks, some of them ceremonially wound with cotton. Corn rested on the head, cheeks, and chest.

In the next room were a dozen ceremonial pipes, or cloud blowers, and a tiny pendant of copper. Both the room and the passage way were very near an interior kiva. From that fact and from the concentration here of so many ceremonial objects, we thought it probable that we had uncovered a cacique, or priest. And, as, at the time of our discovery, basket burials, common enough in the Chaco and elsewhere, had not been reported in this region, we dreamed a dream of a foreigner and a stranger, who had not only ruled in his own lifetime, but was also able, even after death, to insist upon the burial rites of his own people. Later, however, we found evidences of the basket in seven subsequent burials. Moreover, the material taken from the burial mound of Otowi, now in the National Museum, includes the remains of many baskets, similar to those that we found with burials. Evidently, in the wetter climate of Otowi, the baskets had so disintegrated as to disguise their actual use. In the drier climate of the Chaco, they have been

perfectly preserved. Probably, basket burials at Otowi were survivals of customs, or religious ceremonials, that originated long before the migration of these people southward to the Pajarito.

Another interesting day was marked by the finding of eighteen bone flutes and whistles, together with a perfect and beautiful little prayer-meal bowl, all in a single room. The bowl's rim was notched in the making in four places, each, evidently, a guide to each of the four directions. The decoration was of conventionalized dragonflies, very similar to those so frequently found in present-day Navajo art. Diagonally opposite to this room was another in which was found another prayer-meal bowl, also notched, decorated with lightning water serpents issuing from clouds. In this house, we found the skulls of four deer and the jaw bones of seven. We named these rooms the "House of the Musician" and the "House of the Hunter," respectively, privately noting that each of these successful gentlemen had been of a religious turn of mind.

Another happy day resulted from the discovery, already reported in the *Anthropologist*, of a little god, or devil, perhaps, in its own niche in the wall. It is made of clay, originally painted pink, but now black with soot. In a room nearby were eight similar niches, but all were empty. Numbers of stone idols have been discovered, but up to date this is the only one of clay. It confirms the account given by Espejo of household idols and of rooms set apart for their worship.

But the greatest day of all, both for the Indians and myself, occurred shortly before we closed camp, last summer. It was the discovery on the wall of the largest room excavated of a fresco in color of a mountain lion. The figure is colored yellow ochre, outlined with black. Evidences of color in the plaster of caves, both at Otowi and at Frijoles are common. But, so far as I can find out, this is the only certainly prehistoric colored drawing ever found in the United States.

The room measured twelve by eighteen feet. To the east, was a fine ceremonial fireplace near a door way, much too small to have been used by living Indians, however well it may have served the spirits of their ancestors!

FEDERICO GONZALEZ SUAREZ

By MARSHALL H. SAVILLE

Federico Gonzalez Suarez, Archbishop of Quito, Ecuador, died at the age of seventy-three, on December 1, 1917. Doctor Gonzalez Suarez was unquestionably the foremost native South American historian, and his History of Ecuador will always stand as a fountain of information, and a model of careful and judicious research. The late Archbishop was born in Quito, April 13, 1844. His mother was a native of Quito, and his father, who died when the son was quite young, was a Colombian. At an early age the youth decided to enter the church, and when twenty-one he became a member of the Order of Jesuits. He taught literature, philosophy, and the humanities, successively in the Jesuit colleges of Quito, Guayaquil, and Cuenca. He left the Jesuit Order in 1872, and became attached to the church at Cuenca as Secretary of the Curacy. In his early youth Gonzalez Suarez gave evidence of his literary ability, and he soon became a prolific writer on matters pertaining to the church. After his retirement from the Jesuit Order the opportunity for labors in historical research came to him, and the first essay, an archaeological one, was begun while he resided in Cuenca. In 1853 in the little village of Chordeleg in Southern Ecuador, some ancient tombs were found containing a great treasure of gold and other objects. The site was visited twenty years later by the priest, who gathered data for his important *Estudio Historico Sobre Los Cañaris*, published in 1878. Although the bulk of his later literary work was more largely historical, his predilection was towards the problems of archaeology. As one of his fellow countrymen has written,

in the midst of the most arduous episcopal work he turned from time to time with love to his favorite study, and a little before his death was still bringing together the fruit of his latest lectures and studies, in the form of advice to the youth who were following in his footsteps.

In 1881 he completed and published a single volume of an ecclesiastical history of Ecuador. The mass of material which he had gathered in the preparation of this work caused him to suspend the completion of the history on the lines begun, and to undertake the preparation of a broader and more comprehensive history of his native land. In the furtherance of this task he went to Europe in 1884 where he remained for nearly three years, working in the archives and libraries of Spain and Portugal. In these studies he examined more than two hundred documents in the library of the Royal Academy of History in Madrid, and more than one thousand bundles of manuscripts in the Archives of the Indies in Seville. Of these manuscripts he made copies with his own hand of all of those which related to the subject under investigation. On his return to Ecuador he at once began his history, although still engaged in church work, and from the years 1887 until 1903 was engaged in its publication. This monumental work was issued in seven volumes, comprising about three thousand pages, and volume one was printed in 1890. Besides the purely historical matter it included an archaeological atlas and text not numbered in the series. With the completion of the seventh volume, which takes the narrative down to the year 1809, a period just before the time of the independence of Ecuador, the author abandoned the completion of the work, due in part to increasing clerical duties, and also to failing health. In 1895, he became Bishop of Ibarra, and in 1906 he was consecrated Archbishop of Quito. While living in Ibarra he wrote his treatise on the Aborigines of Imbabura and Carchi, and after becoming Archbishop he found time to continue his archaeological studies. As has been said he continued to take an active interest in this branch until his death.

The writer became acquainted with the Archbishop in 1906, and owes much to his kindly interest and advice, in the prosecution of his archaeological researches undertaken at that time through the financial coöperation of Mr. George G. Heye. A man of quiet and courteous manners, with the mien and face of a scholar, it was always a great pleasure and privilege to be received by the Archbishop in his simple and austere study. The impression given was

that of meeting a friend and fellow student. With an accurate reading knowledge of the leading languages, he was a painstaking student of the studies issued in the English tongue. His history was written without the least bias as a prelate. He searched for the truth, and on the occasion of the appearance of the fourth volume of his history in 1893, in which he had treated at length of the delinquencies of certain of the fathers of one of the orders in Quito, based on evidence found by him in some documents in Spain, a storm of protest was raised by some of the more bigoted members of the Ecuadorian clergy. Among these was the German-born Bishop of Manabi. The justice of his fearless attitude in the search for historical truth was recognized by the Pope, as evidenced by his appointment as Bishop of Ibarra two years later. The merits of this great and impartial history are appreciated in Spain and Latin America, but it is not as well known in this country as it deserves. Dr. Gonzalez Suarez was the father of Ecuadorian archaeology, and as a prose writer and poet he ranks high among the Spanish American writers of the nineteenth century. We do not hesitate to place his history as a product of Latin-American genius and acumen, with the history of the Mexican, Orozco y Berra, who was one of the most gifted historians of either North or South America. In the death of the distinguished Archbishop, Ecuador has lost one of her greatest sons, and Latin America one of her most brilliant men of letters.

The following partial bibliography inadequately represents the literary labors of Dr. Gonzalez Suarez. It includes only those titles which are in the library of the writer.

1878. Estudio Sobre Los Cañaris, Antiguos Habitantes de la Provincia del Azuay en la Republica del Ecuador. viii, 55 pp., 5 plates. Quito, 1878.
1881. Historia Ecclesiastica del Ecuador desde los Tiempos de la Conquista hasta Nuestros Dias. Tomo primero 1520-1600. 12 unnumbered, xxxvii, 412 pp. Quito, 1881. This is the only volume published.
- 1890-
1903. Historia General de la Republica del Ecuador. Nine volumes. Volume I, xvi, 319 pp., Introduction, and Ecuador in Pre-Spanish times. Archaeological Atlas: Text, xvi, 210 pp. Plates, xxiv. Volume II, viii,

- 480 pp., *Discovery and Conquest 1530-1564. Volumes III to VII, The Colony, Ecuador under the Government of the Kings of Spain, 1564-1809.* The entire work comprises about three thousand pages.
1901. *Recuerdos de Viaje ó Cartas acerca de Roma, España, Lourdes y Colombia.* xii, 203 pp. Frigburgo de Brisgovia, 1901. Second edition.
1903. *Los Aborígenes de Imbabura y del Carchi.* 71 pp. Author's edition of twenty-five copies reprinted from the *Anales de la Universidad Central de Quito*, Numbers 118-129. Quito, 1902-1903.
1904. *Prehistoria Ecuatoriana Ligeras Reflexiones sobre las Razas Indígenas que Poblaron Antiguamente el Territorio Actual de la República del Ecuador.* vi, 87 pp., 5 plates. Quito, 1904. The appendix contains several vocabularies and texts of the native languages of Ecuador. On plate III are illustrated a number of the gold objects known as the "treasure of Sigsig" now in the Museum of the American Indian (Heye Foundation).
1906. *Memoria Histórica sobre Mutis y la Expedición Botánica de Bogotá en el Siglo Decimo Octavo (1782-1808).* xxxii, 129 pp. Quito, 1906. A first edition was published in 1888.
1907. *Un Opusculo Inédito de Don Francisco José de Caldas.* xvii, 25 pp., 3 maps. Quito, 1907.
1910. *Los Aborígenes de Imbabura y del Carchi.* Text, xiv, 145 pp. Atlas in long folio of xxxxi colored plates. This is a rewritten and extended second edition of the work published in 1903. The number of copies of the Atlas, published for the first time, was limited to fifty.
1910. *Disquisición Crítica*, in the *Biografía de Don Pedro Vicente Maldonado* by Antonio C. Pérez, pp. 59-60 of the same. Quito, 1910.

MUSEUM OF THE AMERICAN INDIAN, HEYE FOUNDATION,
NEW YORK.

BOOK REVIEWS

METHODS AND PRINCIPLES

The Causes and Course of Organic Evolution; A Study in Bioenergies.

JOHN MUTRHEAD MACFARLANE, D.Sc., Professor of Botany, Director of the Botanic Garden, University of Pennsylvania. The Mac-Millan Company. New York, 1918. Pages ix, 875. 29 text figures. Price, \$4.00.

In this volume we have the contribution of a botanist to the more recent trend of evolutionary thinking. In part the book parallels Osborn's "Origin and Evolution of Life," but the scope is broader and the treatment more detailed and technical. Emphasis is placed on the causal factors of evolution but the purely historical phase is by no means neglected.

The thesis is the continuity of all matter and manifestations of energy both inorganic and organic. In turn the author discusses the formation of inorganic matter, the formation of the world, the origin of life, the evolution of plants, the evolution of animals, and human evolution, both physical and mental.

As factors of evolution Heredity, Environment, Pro-environment, Selection and Reproduction are named. Special emphasis is placed on the effects of Reproduction and Pro-environment or harmonious interactions.

The evolution of energy itself is discussed. First we have the crystalloid energies energizing inorganic bodies and known as thermic, lumic, chemic and electric. Colloid energies energize organic bodies and are known as biotic, cognitive, cogitic and spiritic. The energy transitional from inorganic to organic bodies is known as duplo-electric.

Biotic energy is associated with protoplasmatin and is found in non-nucleate organisms. Biotic energy also occurs in higher nucleate plants and animals in combination with cognitive energy, the seat of which is chromatin. In the higher animals we have biotic and cognitive energy combined with cogitic energy which is associated with neuratin or Nissl's granules. In man we have a still higher energy designated as spiritic associated with spiritin, hypothetically located in the cerebral substance near the frontal sinuses.

The work culminates in the author's philosophy of life. Morals, religion, coöperation and competition are subjected to an evolutionary analysis. Naturally, many of the details are speculative. In matters of phylogeny there is some room for differences of opinion. It does not seem that Professor MacFarlane has given due weight and importance to the paleontological evidence for a reptilian ancestry of the mammals. However, this does not detract from the main thesis of the book.

Chapters xx to xxx should prove of interest to anthropologists.

LOUIS R. SULLIVAN

The Frontiers of Language and Nationality in Europe. LEON DOMINIAN.

Published for the American Geographical Society of New York by Henry Holt and Co.: New York, 1917. Pp. xvii, 375, 9 plates, 67 figures.

This is an excellent book, which no anthropologist, concerned at all with Europe, can afford to dispense with as a work of reference. It is strictly impartial in its presentation of evidence, critical in its interpretations, and for a war-time work concerned with issues of the war, surprisingly fair in its attitude. In addition, it is excellently written. To the scientific student its chief value will be as a convenient and authoritative compilation. The maps—all of the "plates" and many of the "figures" are such—are of high grade: without exception they show clearly the particular points which they illustrate. An introduction by Madison Grant emphasizes the prevailing lack of race consciousness in Europe and the circumstance that language is the essential factor in the creation of national unity and nationality. The author and the Society are alike to be congratulated on this work.

A. L. KROEBER

[Collected Papers in] *Analytical Psychology.* C. G. JUNG. Authorized Translation edited by Dr. CONSTANCE E. LONG, New York: Moffat, Yard and Co., 1916.

The Psychology of the Unconscious [Wandlungen und Symbole der Libido].

C. G. JUNG. Translated by Dr. BEATRICE M. HINKLE, New York: Moffat, Yard and Co., 1916.

These volumes are of interest to anthropologists for two reasons. One is that, in some form or other, psychoanalysis has come to stay. The extravagances of some of its followers, and possibly an excessive confidence on the part of all of them, may vitiate much of the present status of their science. For instance, its future center of gravity may

conceivably lie in the non-sexual rather than sexual field. But certain of its findings, as to the conversion rather than extinction of repressed desires, for instance, and the significance of dream material, have surely become a permanent part of general psychology and therefore relate to that form of activity which underlies all social phenomena and which the anthropologist can never afford wholly to ignore.

The other point of contact is the assumption, apparently typical of the school, that the symbols into which the "libido" converts itself, are phylogenetically transmitted and appear socially. The machinery of this assumed process is not examined. Its reality is considered established by the adduction of examples which may be so interpreted. Now if the psychoanalysts are right, nearly all ethnology and culture history are waste of effort, except in so far as they contribute new raw materials. If, on the other hand, current anthropological methods and the psychological assumptions underlying them are correct, the phylogenetic theories of Jung and his collaborators are only a mistaken excrescence on their sounder work. Mutual understanding will not progress as long as the two tendencies go their conflicting ways in ignorance of each other.

Of the two volumes, the first is in reality the more systematic. The series of papers gives an excellent cross-section of the modern psychology of the unconscious as represented by one of its two leading schools. The second volume is saturated with phylogenetic interpretations without examination of their foundations. Both translations are good.

For those whose patriotic sensibilities are keen, it may be remarked that while Jung writes in German, he is a Swiss and head of the Zurich school.

A. L. KROEBER.

NORTH AMERICA

In the Alaskan Wilderness. GEORGE BYRON GORDON, Sc.D., F.R.G.S.

The John C. Winston Company: Philadelphia, 1917. 247 pp. 52 illustrations, 3 maps.

This volume is the narrative of a journey in Alaska from the Upper Yukon southwestward to the headwaters of the Kuskokwim and thence down that river to the sea. The party was composed of the author and his brother, Lieutenant MacLaren Gordon, who fell on the battle line in France, October 21, 1916. The explorations upon which this volume is based were made in 1907. The main portion of the book comprises ten chapters, in narrative form, covering the journey from start to finish. The style is charming, simple, and direct; the reader is carried along without being wearied by long and tedious details, but always has before

him a running picture of the country and the life by the way. Strange to say, a part of the territory traversed is so little known that some of the observations and the map at the end of the volume constitute the first definite geographical knowledge we have for the region around the headwaters of the Kuskokwim.

A few straggling Indians from the interior were encountered near the head of the Kuskokwim, but the author had neither the time nor the linguistic equipment to gather much information as to their culture. When about 275 miles from the sea the first Eskimo village (Sikmiut) was observed. From a brief observation, supplemented by collecting, the author found the culture here to be mixed with that of the Indians farther inland. Also he thought he could detect both types in the physical features of the inhabitants. As the journey proceeded to the sea, five other villages were visited, but these the author considers pure Eskimo.

An appendix of eighty pages contains some data for traveling on the Kuskokwim and in Alaska which will no doubt be useful to future visitors to the country. This is followed by a general discussion of the population of Alaska. It is to be regretted that the author does not discuss the data returned by the census of 1910, particularly that for the region he traversed, for this would have given us a check upon at least one geographical unit. There is, however, a very satisfactory digest of the Kuskokwim culture illustrated by many plates of specimens collected on the journey.

A brief vocabulary closes the volume. Besides its many other estimable qualities this volume gives one a clear picture of the environmental conditions surrounding Eskimo culture in the Alaskan river deltas.

CLARK WISSLER

ASIA

Aboriginal Siberia, A Study in Social Anthropology. M. A. CZAPLICKA, Somerville College, Oxford. With a Preface by R. R. MARETT, Reader in Social Anthropology in the University of Oxford. Oxford: Clarendon Press, 1914. 8vo, pp. xvi, 376, 16 pls. \$5.60.

Though this book was issued several years ago, it has somehow eluded the notice of reviewers in this country, but a belated comment seems better than none. Miss Czaplicka was one of the many students who derived inspiration from Dr. Marett's anthropological teaching, and the present work was undertaken at his suggestion prior to a year's trip to Siberia. It is best characterized as a painstaking compilation by an enthusiastic novice. A Pole by birth and conversant with the Russian

language, the author has rendered accessible a considerable amount of valuable material that would otherwise have remained wholly unknown to non-Slavic anthropologists. Her method of presentation does not invite continuous reading, but as a work of reference its usefulness cannot be doubted, especially for those who are obliged to contend with mediocre library facilities. For Miss Czaplicka does not limit herself to Slavic publications, but endeavors to present a complete account of non-material Siberian culture on the basis of all published material.

While Miss Czaplicka's essay must be hailed with pleasure, there are, however, certain obvious deficiencies which one hopes to see remedied in supplementary publications since second editions seem beyond the range of possibility nowadays. The material culture of northern Asia is singularly replete with interesting information and suggestive problems, which might at least have been sketched in an introductory chapter without undue expansion of the volume. A more serious fault results from the compiler's naïveté in ethnographical matters, or it may be from her lack of interest in the subject of historical connections. For example, she mentions and briefly characterizes the Whale festival of the Koryak (p. 295), but she does not specifically point out the historical significance of its dominant features. It is also not clear why so important a topic as aboriginal literature is ignored. Here again the subject-matter has an extraordinary intrinsic interest, as readers of Radloff's *Aus Sibirien* will recall, while the historico-ethnic problems involved are of tremendous importance. It is also to be regretted that Miss Czaplicka does not take a definite stand on the question of American-Asiatic connections, a subject brought prominently before the anthropological public since the Jesup Expedition. Finally, the publishers may be implored to provide for more adequate maps in future publications of similar scope; this deficiency unfortunately is shared by too many ethnological works of larger scope, definitely detracting from their value.

The foregoing remarks are not offered in the way of carping criticism but in order to define the character of the book from the anthropologist's point of view. It is distinctly a meritorious compilation of raw material, but the specialist must not expect from it an integrated picture of Siberian culture.

ROBERT H. LOWIE

SOME NEW PUBLICATIONS

Balfour, Henry. Some Ethnological Suggestions in regard to Easter Island or Rapanui. (Folk-Lore, xxviii, 1917, pp. 356-381.)

Hague, Eleanor. Spanish-American Folk-Songs (Memoirs of the American Folk-Lore Society, vol. X). \$3.50, to members \$3.00.

Jones, William. *Ojibwa Texts*. Edited by Truman Michelson. Publications of the American Ethnological Society. E. J. Brill, Ltd., Leyden, 1917. (G. E. Stechert & Co., New York agents.) vii, 501 pp.

Ten Kate, Dr. H. *Mélanges anthropologiques*, IV-VI; Polynésiens (*L'Anthropologie*, xxvii, 1916, pp. 393-406, 569-583); Indiens de l'Amérique du Nord (*ibid.*, xxviii, 1917, pp. 129-155; 369-401.)

Laufer, Berthold. The Vigesimal and Decimal Systems in the Ainu Numerals; with some Remarks on Ainu Phonology. (*Journal of The American Oriental Society*, vol. 37, 1917, pp. 192-208.)

Id., Origin of Tibetan Writing. (*Ibid.*, vol. 38, 1918, pp. 34-46.)

Id., Totemic Traces among the Indo-Chinese. (*Journal of American Folk-Lore*, xxx, 1917, pp. 415-426.)

Id., The Language of the Yüe-chi or Indo-Scythians. Chicago: Donnelley & Sons Co., 1917. 14 pp.

Montgomery, J. A., *ed.* Religions of the Past and Present. J. B. Lippincott Co., Philadelphia, 1918. 450 pp. \$2.50. (Collaborators: F. G. Speck, W. Max Müller, Morris Jastrow, Franklin Edgerton, Roland G. Kent, W. W. Hyde, G. D. Hadzsits, Amandus Johnson, W. R. Newbold, Arthur C. Howland.)

Michelson, Truman, *ed.* (See Jones, William.)

Parker, Arthur C. A Prehistoric Iroquoian Site on the Reed Farm, Richmond Mills, Ontario County, N. Y. (*Researches and Transactions of the New York State Archeological Association*.) Morgan Chapter, Rochester, N. Y., 1918. Vol. 1, No. 1, pp. 1-41, 14 figs.

Routledge, Scoresby, Mrs. The Bird Cult of Easter Island. (*Folk-Lore*, xxviii, 1917, pp. 337-355.)

Smith, Elliot G. Ships as Evidence of the Migrations of Early Culture. Manchester: University Press, 1917. 40 pp.

Will, George F. and Hyde, George E. Corn among the Indians of the Upper Missouri. The Wm. Harvey Mines Co., Inc., St. Louis, 1917. 323 pp., 28 ills.

Zayas, de M. African Negro Art, its Influence on Modern Art. Published by Modern Gallery, 500 Fifth Ave., New York, 1916. 44 pp., 31 pls.

DISCUSSION AND CORRESPONDENCE

PUEBLO TRADITIONS AND CLANS

DR. SWANTON and Dr. Parsons, having done me the honor of reviewing my Zuni Kin and Clan in a recent number of the *Anthropologist*, have mingled commendation and stricture. While controversy is ordinarily perhaps the least fruitful form of scientific literature, definition of issues is always stimulating; and in this spirit I reply to them.

Dr. Swanton's dissent is on the grounds of some considerable probability of historical actuality for the Hopi clan migration legends. The Hopi having Pueblo institutions but being Shoshonean in speech, that is as it were non-Pueblo, it is clear that there has been a change due to contact. Hopi tradition narrates an amalgamation of northern, eastern, and southern peoples, presumably of Shoshonean, Pueblo, and perhaps Piman affiliations. According to Dr. Swanton, this tradition is more plausible than the assumption that a Shoshonean group, coming to be within the Pueblo area, adopted Pueblo institutions; or that an outlying Pueblo group adopted Shoshonean speech from its neighbors.

As between the last two alternatives, civilization in the vast majority of cases changes so much more rapidly than language, that the alternatives practically resolve into the supposition that a Shoshonean people originally non-Pueblo in customs became puebloized. Whether the occasion was a change of residence on their part, an intensification of Pueblo influence, or an alteration of political and economic connections, does not now matter. The difference is that this view involves essentially only a contact or mixture or replacement of civilization; Dr. Swanton's view, a physical mixture of peoples themselves.

Now, of these two interpretations, the former is the simpler, because it is obvious that civilization does often spread and change, and this interpretation assumes nothing more. Dr. Swanton's theory has to retain the factor of alteration of culture, and adds to it the factor of amalgamation of peoples. On the principle of simplicity and sufficiency of means, this additional factor is gratuitous. In fact, it is probable that it would have occurred to no one to bring in this gratuitous factor if the Hopi themselves had not done so. Personally, I resent quite vigorously the implication that a Hopi is a better ethnologist, even on matters Hopi,

than I am. If he is, we might as well accept the German view of German culture, on the ground of the Germans knowing more about it.

The methodological question is, which is to count for more: first-hand information or a willingness and ability to apply critical methods? The layman's impulse is to fear circumstantial evidence and trust the eye witness. But the whole progress of science has, from one aspect, been a history of the replacement of direct testimony by testimony that is circumstantial, or, as we put it, indirect and analyzed and subject to control. If any native or Caucasian is to be recognized as an ethnologist about the Hopi because he has made or seen the snake-dance and eaten piki bread all his life and listened to traditions from an old priest, ethnology is not and will never be a science nor even quasi-science.

The objection to Dr. Swanton's view may be put in another form. He attempts to explain a cultural phenomenon, the Hopi or Pueblo clan system, not purely in terms of its antecedent or causative cultural phenomena, but also in terms of the human beings that are the carriers of the culture. This is still a rather common procedure, unfortunately, in spite of the substantial accord to the contrary among even those of the advanced currents of ethnological thought that otherwise diverge most widely. If the carriers of culture are to be introduced as a factor into the reckoning of culture, then the hereditary strains of these factors must also be introduced, and the environment that moulded these hereditary strains, and the physiological reaction on their systems of the food they ate or the actinic rays that shone on their skins in the glare of the southwestern sun. It is true that we want a door and not a wall between ethnology and the underlying sciences. But we are either doing ethnology, in which case our business for the time being is to shut the door; or we are investigating the relation of ethnological data to data of other kinds, in which case the door should be as wide open as possible. Dr. Swanton is an ethnologist, and I am sure would be horrified at the prospect of having to do his ethnology in a chamber of which geographers and biologists and chemists could at any moment take possession, even to the very desk at which he was working. I am sure he really thinks he is entitled to privacy. But for some reason the final click of the latch disconcerts him; so in the present case he tries to compromise by leaving the door just ajar.

To return once more to the Hopi, Dr. Swanton restates my data as to the strength of the several clan groups in various pueblos in order to argue that these show rather close coincidence in detail with tradition and can therefore be accepted as at least partly confirmatory of it.

What his table however shows of a broader scope is two things. First, the populational representation of the clan groups is more similar between Zuñi and the Hopi at large than between Zuñi and the one Hopi pueblo of Sichumovi to which native tradition attributes a Zuñi origin. Evidently modern clan strength is no index of past movements and amalgamations of population; or these movements and amalgamations have been different from what tradition represents. Second, about four per cent of the Hopi of today belong to clans alleged to have come from the north, that is, to have a Paiute or Shoshonean origin. Ninety-six per cent are attributed to the east and south, or a Tanoan and Piman source. Yet Hopi speech is a hundred per cent Shoshonean. How did the four per cent come to impose their pure language on the ninety-six? Such an event is conceivable, but extraordinary and unlikely. It is certainly more conservative to assume that tradition is unreliable, that a very much larger proportion than one twenty-fifth of the Hopi are really Shoshonean in origin, but that tradition has come to ascribe a non-Shoshonean origin to the majority of the Shoshoneans in the population.

Both these cases indicate very strongly that Pueblo tradition is fluid and shifting. They also suggest what I have been at pains to point out again and again in my paper: namely, that the generic clan concept is apparently remarkably uniform through the Pueblo area and therefore presumably deep rooted and of some antiquity, but that the actual clan organization, the forms which this concept or system assumes in practice, are subject to rapid secondary modification. Here is the real difference between Dr. Swanton and myself in the face of the specific Pueblo problem. I hold both clans and tradition to alter readily. He is appreciably nearer the native attitude that both clans and traditions are immutable. That definition of the difference between us seems to me fair; and with it we can leave the subject.

Dr. Parsons' criticism is along three lines. The first is that I have separated the religious and the social aspects of Zuñi clans too sharply. This is probably true. A clan is after all, formally and in native consciousness, a social device. But when this social device seems to function much more actively in ceremony than in purely social relations, as seems to me to be the case at Zuñi, the fact is certainly notable. I have therefore tried to bring it out clearly, and if in so doing I have overstressed the situation, the fault has been due to a desire to subordinate the less significant to the more significant. To this extent I gratefully admit Dr. Parsons' strictures as being in order. "Social" and "ceremonial" are of course only facets of the same thing, not separate com-

partments. Still, they are distinct facets. And just because clans are at bottom pieces of social machinery and have been generally supposed to be the warp of the social fabric as such, whereas at Zuñi the bulk of strictly social functioning takes place through the medium of kinship and the house, while the functioning of the clan is to a surprisingly large extent expressed in the domain of religious ceremony, it seems to me that the formulation which I have made is significant and in the main justified. The very facts which Dr. Parsons adduces in her discussion appear on the whole to accentuate the same formulation.

That I have passed almost wholly over the economic relations of Zuñi clans is true, but due only to ignorance and the impossibility of covering all phases of a rich culture in two brief periods of investigation. We need data on the economic life not only of the Zuñi, but of all the pueblos and of most American tribes. There is probably no phase of native life that has been so unreasonably neglected by American anthropologists. I have merely sinned with the majority. Still it is hardly likely that the economic functions of clans can be of primary importance at Zuñi, else between Cushing, Stevenson, Dr. Parsons, and myself, something of moment would have emerged. I think it characteristic of the situation that the one thing which Dr. Parsons is able to point out as being felt by the Zuñi as specifically clan owned, namely the *ettowe* or fetishes, are religious property.

As to the third charge, that I have undervalued the attitude of the Zuñi themselves toward clanship, I have nothing to say except that I have tried to depict their attitude as I found it, and that the clan seemed to rest surprisingly lightly on their consciousness. This, however, being a matter of valuation, is one of integration, rather than one to be advanced by an analytic discussion of individual items, which might go on endlessly. If my judgment is in error, it remains for Dr. Parsons or some other investigator to assemble all possible data on the point and to strike a new balance which shall bear on its face the marks of being more justly proportioned.

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AN HISTORICAL NOTE ON THE WESTO INDIANS

WHO were the Westo Indians of the seventeenth century South Carolina frontier? In view of the rôle played by this folk in the period of the first settlement the question has an interest for the historical student measured neither by the size nor by the permanent importance of the tribe. It is, however, one of those questions which he expects the

ethnologist to answer for him. In this instance an answer has in fact been given which bears an aspect of plausibility, and which is stamped with the authority of well-warranted scholarship. According to J. R. Swanton, who has extended his researches in the early history of the southern Indians to the manuscript sources, including the South Carolina colonial archives, the Westo were almost certainly identical with the eastern group of the Yuchi. In this conclusion F. G. Speck, the principal student of the Yuchi, has concurred.¹ It is the purpose of this note to set forth a number of reasons for rejecting Dr. Swanton's theory, and to suggest another solution. In so far as the deductions are not strictly historical they are put forward tentatively, and it is hoped will receive the criticism of professed ethnologists.

What are the ascertainable historical facts with regard to this tribe? When the first settlers reached the South Carolina coast in 1670 they found the small tribes from Port Royal to Kayawah terrorized by "another sorte of Indians that live backwards in an intier body & warr ag^t all Indians . . . haveinge gunns & powder & shott"—"a ranging sort of people reputed to be the Man eaters." Such was their prestige that the Savannah river, on which they were seated, though not at the mouth, was called the "Westoe bou signifying the enemies River." The universal fear which the Westo inspired greatly facilitated the planting of the Charles Town settlement, by ensuring the friendship and coöperation of the coast Indians. Until 1674 the colonists were involved in small wars with the Westo. In that year Henry Woodward, agent for the proprietors in the Indian trade, made a "discovery" of their chief settlement, which he called in his relation "Hickauhaugau." This he described as a palisaded town on the western bank of the Savannah, enclosed in a sharp bend of the river (perhaps in present Screven county, Georgia). He learned that the Westo were enemies of the Cherokee and Kawihta and Kasihta, as well as of the tribes of the coast; and that they had goods from the north (Virginia). Woodward opened a trade with them, and from 1674 to 1680 the Westo alliance formed the cornerstone of the South Carolina Indian system. They alone were supplied with arms,

¹ "Handbook of American Indians," *Bureau of American Ethnology, Bulletin* 30, vol. II, p. 936 (article on Westo by Swanton); *ibid.*, p. 1003 (article on Yuchi by Speck). See also Swanton, "De Soto's Line of March" in *Mississippi Valley Historical Association Proceedings*, vol. V, p. 153; Swanton and J. R. Dixon, "Primitive American History" in *American Anthropologist* (N. S.), vol. 16, p. 383; and Speck, "Ethnology of the Yuchi Indians" in *Anthropological Publications University Museum, University of Pennsylvania*, vol. I, no. 1. In correspondence Dr. Swanton has kindly given me a fuller statement of his views than he has yet published.

and were expected to protect the province by overawing the Spanish Indians and all other potential enemies. But the alliance proved unstable. The proprietors insisted upon a monopoly of the trade with the distant Indians. Meanwhile the Westo renewed from time to time their raids upon the settlement Indians, with whom the colonists traded. Out of this situation, which created a sharp conflict between the interest of the proprietors and the interest of the private traders who controlled the provincial assembly, arose the Westo war of 1680-1681. In 1680 the settlers engaged the Savannah (the eastern Shawnee, probably recent immigrants from the west) to expel the Westo from the province. Despite the opposition of the proprietors this was accomplished: so thoroughly, indeed, that in 1683 it was reported to the proprietors that scarcely fifty Westo remained alive and those in scattered bodies.¹

From several eighteenth-century maps it has been known that at some time after their defeat the remnant of the Westo retired among the lower Creeks, first on the Ocmulgee, later on the Chattahoochee.² When did this migration occur? On this point the maps have furnished no clue. For a decade after 1683, moreover, the South Carolina records are silent with respect to the Westo. In 1693, however, two entries in the journals of the Commons House of Assembly throw a sudden flash of light upon their fortunes after their expulsion, and upon the more involved question of their identity. An analysis of this unexploited contemporary material will follow a discussion of the Swanton-Speck hypothesis: that the Westo were identical with the Yuchi.

The basis of this identification is entirely circumstantial: it rests upon an argument from location. From Woodward's relation and from the Indian references to the "Westoe bou" it is certain that the Westo were seated on the Savannah river in the late seventeenth century. It is equally well established that the Savannah river from Ebenezer creek near its mouth to fifteen or twenty miles above present Augusta, Georgia, was once the habitat of an eastern division of the Yuchi. Secondly, there is cartographical evidence that the Westo and Yuchi villages among the lower Creeks were immediately adjacent. Neither set of facts is in itself conclusive; together they have been thought to establish the probability of the identity of Westo and Yuchi.

¹ *Collections of the South Carolina Historical Society*, vol. v, pp. 166-168, 194, 378, 446, 456-462. *Grand Council Journals* (MSS. Columbia, S. C.), *passim*. Correspondence of proprietors, especially letter of September 30, 1683, to governor, deputies and parliament, in Public Record Office, *Colonial Entry Books*, vol. xxii, 16. (Transcripts, Columbia, S. C.)

² See maps of Moll (1720), Bellin (1744), and Mitchell (1755).

But the argument presupposes (a) that the Yuchi migration from the west occurred before 1670; and (b) that a considerable body of Westo remained in South Carolina after their supposed expulsion.¹ The latter surmise is directly contrary to the positive testimony of contemporaries: the Westo, apparently never a large body of Indians, were clearly "ruined" by the war. For (a) there is no support, save an argument from silence. If the migration of the Yuchi had occurred after the settlement of South Carolina, it may be asked, why was it not mentioned by any contemporary writer? But the argument from silence (wisely employed with caution, in view of the fragmentary character of the sources) may be used more effectively in rebuttal than in support of the contention. If the eastern Yuchi had been settled on the Savannah river in the late seventeenth century is it likely that no mention of their presence there would occur in the South Carolina records until 1707, especially in view of the fact that from shortly after the Westo war the principal route of the Indian traders lay directly through the region later known to be their habitat? References to the tribes of the Savannah river region (the Yamasee, the Palachacola, the Savannah, and, after 1704, the Apalache) abound in the accounts of the trade, but before 1707 there is no mention of Yuchi on that river under any conceivable variant of their tribal name. A single allusion to the Yuchi appears in the seventeenth-century records. In 1691 the assembly laid an embargo upon the trade with the distant tribes. The traders were given specified periods within which to bring in their goods. Included in the ban was trade with the "Attoho Kolegey"—probably the Yuchi under a disguised form of their Algonquian name (Tahogale). A trans-Appalachian tribe was meant, for these traders, like the Cherokee dealers, were allowed from March to September of the following year to close their trade, whereas the traders with the Kasihta and the Kawihta were granted only the four months then ensuing.

In 1707 there is for the first time definite indication of an eastern settlement of the Yuchi. It was proposed in that year that a force of Indians be assembled for the protection of the province in an expected emergency, to include "100 from the Savannahs and Appalachees and Tohogoligo." The apposition of the names clearly points to neighborhood. The numbers of the eastern Yuchi must still have been small, for they were not mentioned in the elaborate account of the Indians

¹ Speck in the "*Handbook of American Indians*," vol. II, p. 1003, says: "The early writers also state that the Westo were driven out of their country in 1681 by the Savannah (Shawnee), but this must mean only a part of them."

"under the protection of this government" furnished to the Board of Trade in 1708, nor on the Nairne map of 1708 (at least in the Crisp reproduction of 1711). Moreover, Barnwell's North Carolina expedition in 1711, which was accompanied by 56 Apalaches and 87 Yamasees, included only 10 "Hog Logees" or Yuchi. From these facts, attested by contemporary records,¹ and from the known circumstances of the Indian trade it is possible to construct a more tenable hypothesis of the migration of the eastern Yuchi than that which places it anterior to the English settlement. Like the early Cherokee trade of which it was a development, the Tennessee trade must have been carried on largely with the aid of Indian burdeners. It is probable that some of these burdeners remained from time to time at or near Savannah Town, which, in the early eighteenth century, was still the entrepôt of the northern as of the southern trade, and there assisted in carrying on the mountain trade, in the same way that the Apalaches assisted in the trade with the Creeks. A migration which had such an origin would naturally take place too gradually to be certain of mention in the fragmentary colonial records.

In summary, the circumstantial evidence for the identity of the Westo and the Yuchi fails at two essential points: the Westo were certainly not settled on the Savannah after 1681; and the Yuchi were probably not established in any numbers on the South Carolina border before the first decade of the eighteenth century.

If the Westo were not Yuchi, who were they? No answer as satisfying as the one rejected can yet be given. It is possible, however, to identify one unknown with another unknown. The Westo were the Rickahockans of early Virginian history. This was a tribe which entered Virginia from back of the mountains in 1656 and was decisively defeated at the forks of the Pamunkey by Colonel Edward Hill with the aid of the Pamunkeys. After this well-known affair they retired behind the Blue Ridge, to reappear in the more or less fabulous account of Lederer (1669), as dwellers "upon a land, as they term it, of great waves; by which I suppose they mean the seashore." Lederer was probably in contact with a number of Rickahockans, but it is likely that he was mistaken in locating them on his map and in his relation "not far west-

¹ Act of September 26, 1691, in Cooper, *Statutes at Large of South Carolina*, vol. II, p. 66. *Journals of the Commons House of Assembly* (MSS. Columbia, S. C.) under date April 23, 1707. Public Record Office, *Board of Trade Papers, Proprieties*, vol. IX, p. 82 (Transcripts, Columbia, S. C.). Ed. Crisp, "Map of South Carolina" in Library of Congress. *Virginia Magazine of History*, vol. V, p. 393.

ward of the Apalataean mountains"—unless they were at the time separated in two bodies. In that case the customary identification of the "Oustack" with the Westo may still be valid.¹

The proof of the identity of Westo and Rickahockans is as follows: On January 13, 1693, the governor and deputies notified the Commons House of Assembly "that they were informed that some Northern Indians intend next Somer to settle among the Cowataws and Cussetaws" (on the Ocmulgee). The reply of the Commons House next day identified the "Northern Indians" in question. They advised "that all possible means be used to prevent the Settlem^t of any Northern Nation of Indians amongst our Friends more Especially ye Rickohogo's or Westos a people which formerly when well used made an attempt to Distroy us. . . ." The "Hickauhaugau" of Woodward's relation was, then, simply a variant of "Rickohogo" or Rickahockan. After their defeat in 1681 the Westo had naturally retired northward, in the direction from which they had originally come, until, for some reason unknown, they were led to join their former enemies, the lower Creeks, in the last decade of the seventeenth century. Of the competence of the testimony there can be no question. Captain James Moore, one of the committee which carried the message to the governor, had served on a mission to the Westo in 1680, and was, moreover, one of the principal Indian traders of South Carolina.

Thus far all is verifiable. From this point the inquiry enters the realm of conjecture. Who were the Rickahockans, now identified with the Westo? Mooney's supposition that they were Cherokee is untenable: Westo and Cherokee were known enemies. The surmise that they were, the remnants of the Erie (Riquehronnons) is intriguing, but has not found favor with ethnologists.² If neither Cherokee nor Erie there is yet a possibility that they were an offshoot of the Iroquoian group. For the Commons House address of 1693 adds this commentary: "And the Mawhawkes are a numerous, warlike nation of Indians, and strictly aleyd to the Westos. . . ." Much depends upon the interpretation of the expression "strictly aleyd," with regard to which it is dangerous to dogmatize. It is at least conceivable, however, that further researches

¹ Alvord and Bidgood, *First Explorations of the Trans-Allegheny Region*, pp. 155-156 and notes. Neill, E. D., *Virginia Carolorum*, pp. 245-246. Burk, *History of Virginia*, vol. II, p. 107.

² Hanna, *Wilderness Trail*, vol. I, p. 13 and references. Alvord and Bidgood, *loc. cit.*

may confirm the tentative hypothesis that the Westo represent the southernmost migration of the Iroquoian stock.¹

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THE TRIBES OF SIERRA LEONE

THERE are two statements in Professor Starr's review of my books (*American Anthropologist*, vol. 19, p. 286) to which I must take exception, as they appear to be pure perversion of my own statements. My reviewer states (1) that I do not aim at uniformity of spelling in the linguistic portion of the work. It is true that this statement is found in the preface to Part III (stories), and a more cautious mind than my reviewer's might have reflected that it might possibly be intended to refer to the portion of the work to which it is prefixed, and not to the other portion, in particular the dictionary which precedes it.

In point of fact the dictionary is carefully reduced to a uniform system and the defect in it is that words possibly distinguished by tones or by small vowel differences are spelt the same, owing to the fact that some of my material was obtained in England from Ms. and not *viva voce*; it is clear that this is not quite the same thing as Professor Starr has in mind. Uniformity in the spelling of the stories I did not aim at, for the simple reason that dialects exist. Professor Starr apparently thinks that it is desirable to slur over dialectical differences; I do not know whether he has ever attempted to justify this position, which does not commend itself to the majority of those who work at African languages, and does not appear to have any reasonable basis. (2) The second statement is that I have no knowledge of any of the languages that I record. I am at a loss to know where Professor Starr finds this information, if it is not an inference from the preface to my dictionary, in which I refer to the numerous homonyms that further research may distinguish. I have stated above the reason for the uncertainty as to the real position; but I may remark that, even were it otherwise, few linguists have the refinement of ear necessary to establish the minute differences and that this lack does not in any way depend on ignorance of the language.

In point of fact before I left Sierra Leone I had sufficient knowledge of Timne to follow a case in a court of law, and when I came to deal with Schlenker's Mss. in England I was able to read his untranslated stories

¹ Unfortunately there is an almost complete dearth of linguistic data. Besides the name of their town the only other Westo word recoverable is the name of one of their chief men, Ariano. *Grand Council Journals*, April 12, 1680.

without difficulty. For some months too I was in the habit of recording my stories in Timne and making my own translation, appealing to my interpreter only occasionally.

I plead guilty to the charge that I do not understand the remaining languages illustrated in my specimens; I relied largely upon interpreters. I had already however been able in Nigeria to reproduce sentences with such accuracy as to be intelligible to others of the tribe besides my original informant; and I have, wherever possible, made revision of my linguistic material with independent spokesmen an habitual practice. Possibly therefore, even if Professor Starr's statement as to my ignorance were more correct than it really is, some value might attach to my records of native languages. I am at a loss to know why a phonetic transcription of simple sentences should appear to Professor Starr such a difficult matter except on the supposition that he has either never attempted similar work or has tried and failed.

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LONDON, ENGLAND.

MOCCASINS: REPLY TO DR. HATT

MY "critical" remarks were made in as friendly and sincere a spirit as my "friendly" remarks. I would not offer criticism in any other way. Dr. Hatt (this volume, p. 112-115) has not invalidated the facts cited by me and running counter to his hypothesis, which was not clearly developed. The apology which he now offers for it was the very reason why I felt obliged to expose its defects. We have no right to express such an hypothesis without stating all the facts by which it is inspired, and Dr. Hatt himself is now compelled to admit that it was out of place. He cannot expect his readers to guess what he withholds from them, nor can he simply claim credit and credence for his theories by pointing to an arsenal of weapons hidden behind him and not yet examined by others. This procedure is unscientific and pernicious. For the rest, I have no objections if Dr. Hatt will hold on to his speculations, which in my opinion are ill founded. What I offered was some friendly and well-meant advice, and it is every one's perfect right to accept or to reject advice. I am not a missionary bent on making converts.

If I characterized the spelling "Chukchee" as a barbarism, this word was used, of course, in the sense of the Latinist who thus calls any offense against good usage or purity in questions of language, style, or orthography; it does not mean "barbaric" or anything of the sort. Dr. Hatt's outcry framed by quotation-marks is entirely uncalled for.

B. LAUFER

A GRAMMAR AND VOCABULARY OF THE SZINCA LANGUAGE OF
GUATEMALA

THE Szinca¹ language is spoken in the southern parts of the departments of Santa Rosa and Jutiapa in the region extending along the Pacific coast of Guatemala for a distance of about fifty miles, and back from the coast for about the same distance to the sierra. The Rio de los Esclavos runs through this area. Brinton writes that,

there are some reasons for believing that previous to the arrival of the Quiches and Cakchiquels on the plains of Guatemala that region was occupied by this nation.

Calderon considers that the Szinca was the earliest race to occupy this part of the republic, and others have written that it is perhaps the most ancient race in all Guatemala. In his "*Notes Sur L'Americanisme, quelques unes de ses Lacunes en 1900*," Pector calls attention to the importance of the Szinca problem.

At the present time the language is spoken by more than five thousand, but as yet there is scant linguistic material published, or known to be in manuscript, concerning this little-known aboriginal tongue. In 1884, Dr. Brinton published a short paper on the subject, based on material in the Berendt collection. This vocabulary was obtained by Dr. Berendt from Juan Gavarrete of Guatemala, and is listed in the catalog of the Berendt linguistic library as follows: "Vocabularios de la Lengua Xinka de Sinacan por D. Juan Gavarrete (1868) y de Yupiltepeque y Jalapa por Sebastian Valdez, cura de Intiapa Jutiapa (1868)," 15 pp. In 1891 and 1892, Dr. Eustorjio Calderon published in the *Repertorio Salvadoreno*, in volumes v and vi, the results of some linguistic studies made among the Szinca, giving a short grammatical sketch, and a somewhat extended vocabulary. These studies were later republished with some corrections in a pamphlet of seventy-seven pages, under the title, *Estudios Linguisticos. I. Las Lenguas (Sinca) de Yupiltepeque y del Barrio Norte de Chiquimulilla en Guatemala. II. Las Lenguas de Oluta, Sayula, Texistepec en el Istmo de Tehuantepec en Mexico*. This work was printed in Guatemala city in 1908. Dr. Lehmann has published a few words of Szinca in a report of his work in Central America, in the *Zeitschrift für Ethnologie* in 1910. This is all the printed material regarding this language that the writer has been able to discover.

During the winter of 1917 the writer was shown an extensive manuscript relating to this language, in the possession of a priest in the city of

¹ Also spelled Xinca, Sinca and Tzinca.

Guatemala. On returning to the city during the winter of 1908, it was found after a considerable search, having safely passed through the great earthquakes which destroyed the capital, and was purchased for Mr. Charles P. Bowditch.

It is thus far the only extensive work regarding this language, and is a manuscript of the first importance. It is attributed to the year 1770, as it is dedicated by the author, the Maestro Don Manuel Maldonado de Matos to the Archbishop Dr. Don Pedro Cortes y Larraz, who was archbishop at this time. It bears the title: *Arte de la Lengua Szinca con algunas Reflexiones criticas al Arte K'a Kchiquel. Compuesto por el Maestro Dn Manuel Maldonado de Matos Cura propio del Partido de Santiago Sacatepequez. Dedicado al Illmo. Senor Dor Dn Pedro Cortes y Larraz del consejo de su Mag^d Dignisimo Arzobispo de Guattemala*. It is paged in two sections: the first section, numbered leaves 1 to 108, 215 pages of text, is taken up with an introduction and grammar; the second part has 45 numbered leaves, 90 pages, and contains a vocabulary. The entire manuscript contains 305 octavo pages of clearly written material, and is very well preserved. It is now being reproduced in photostat for the use of students.

MARSHALL H. SAVILLE

MUSEUM OF THE AMERICAN INDIANS
(HEYE FOUNDATION),
NEW YORK CITY

PROCEEDINGS OF THE AMERICAN ETHNOLOGICAL SOCIETY

Meeting of October 22, 1917

THE American Ethnological Society Inc. met jointly with the Section of Anthropology and Psychology of the New York Academy of Science at the American Museum of Natural History, October 22, 1917, at 8:15 a.m. A symposium on the "Statistical Growth of Children" was presented by Professor Franz Boas, Mr. Leslie Spier, and Mr. Louis R. Sullivan. Mr. Spier discussed the relation of dentition and stature in the growth of boys, as revealed by a series of casts and measurements made by Professor Boas on 350 school boys at Utuado, Porto Rico, in 1915.¹

Mr. Sullivan presented a summary of a study on the "Growth of the Nasal Bridge of Children."²

The discussion was opened by Professor Boas and participated in by several of those present.

The material reported on was that collected by Professor Boas in Worcester, Mass. A study of the transverse diameters of cross-sections of the nasal bridge showed a gradual decrease from year to year. This decrease in the transverse diameters was interpreted as growth in an antero-posterior direction. Analysis of the averages brought out the following facts which agree with previous studies on growth of other parts of the body:

1. That the girls grow more rapidly from 5 to 14 years.
2. During this period a girl of any age corresponds best in development with a boy from one to three years older.
3. After the age of 14 there is very little growth among girls while the boys continue to grow until 18 or older.

Other data on the height and breadth of the nose were introduced to emphasize the above facts.

Meeting of November 26, 1917

The American Ethnological Society met Monday, November 26, at 8:15 p.m. in the Academy Room, the president in the chair. Dr.

¹ *American Anthropologist*, vol. 20, pp. 37-48.

² *American Anthropologist*, vol. 19, pp. 406-409.

H. K. Haeberlin presented a paper on Sbetetda'q, a Shamanistic Performance of the Puget Sound Salish,¹ illustrated by specimens. Dr. Leo J. Frachtenberg spoke on the Societies of the Quiliute.

Both papers were discussed by Professor Boas and Dr. Goldenweiser. Twenty-eight persons were present.

Meeting of the Board of Directors, January 22, 1918

The Board of Directors met in Dr. Goddard's office at the American Museum at 2 o'clock. Present: Dr. Wissler, Dr. Goldenweiser, Dr. Boas, Dr. Parsons, Mr. Nelson, with Dr. Goddard in the chair.

The Treasurer's report was read and accepted, and referred to an auditing committee, appointed by the President, consisting of A. L. Kroeber and Leslie Spier.

The Editor gave his report on publications. He reported that volume 5 is now completed, as is also volume 7, part 1. Volume 7, part 2 is entirely in type, 560 pages are being made up. Volume 7 is the Jones material, for which the Carnegie Institution will pay \$900. The report was accepted.

The following names were proposed by Dr. Goddard as fellows of the Society: Theodoor de Booy, Dr. Leo J. Frachtenberg, Dr. Walter L. Hildburgh, Max Schrabisch, Miss Ann E. Thomas, Miss Martha Warren Beckwith; and Douglas C. McMurtrie as a member. Mrs. Parsons proposed the name of Mrs. C. H. Sorchan as a member. These nominations were unanimously accepted and the names will be recommended for action by the Board to the Society.

As a nominating committee the Board proposed the following: President, Clark Wissler; 1st Vice President, A. L. Kroeber; 2d Vice President, M. H. Saville; Secretary, R. H. Lowie (M. A. Mallory to act as Secretary in the absence of Dr. Lowie); Treasurer, Elsie Clews Parsons; Editor, F. Boas; Board of Directors (in addition to the above), P. E. Goddard, A. A. Goldenweiser, N. C. Nelson.

The Editor and Treasurer were appointed to prepare a budget to present at the Annual Meeting of the Society.

The meeting was adjourned.

Meeting of January 28, 1918

The annual meeting of the Society was held in the Academy room of the American Museum on January 28, at 8:15. The meeting was called to order by the President, Dr. Goddard. The following members were

¹ This paper appears above pp. 249-257.

present: F. Boas, P. E. Goddard, C. Wissler, M. Fishberg, A. L. Kroeber, E. C. Parsons, L. Spier, L. R. Sullivan, N. C. Nelson, B. T. B. Hyde, B. Weitzner, and M. A. Mallory.

The President called for the report of the Secretary which follows:

SECRETARY'S REPORT

The present membership of the American Ethnological Society, Inc., divided into classes, is as follows:

Life members.....	14
Members.....	12
Fellows.....	63
Anthropological fellows.....	22
Total.....	111

The Secretary has a record of 12 subscribers, but this is not a complete record since the number has been enlarged by our agent, G. E. Stechert and Company. This gives a total of 123 as compared with 121 in January, 1917. The Society has lost one member, Rev. Wm. R. Blackie, by the non-payment of dues.

The Society held the usual number of meetings during the year at which the following papers were presented:

(January) N. C. Nelson, The Southwest Problem.

(February) Tylor Memorial Meeting in honor of Sir Edward Burnett Tylor. Clark Wissler, Tylor and Material Culture. Robert H. Lowie, Tylor on Diffusion and Adhesions. A. A. Goldenweiser, The Doctrine of Animism.

(March) Herbert Lang, Dances of the Natives of the Belgian Congo. Elsie Clews Parsons, The Office of Governor at Zuñi.

(April) Prof. S. A. Smith of the University of Sydney, The Talgai Skull.

(October) Franz Boas, Leslie Spier, Louis R. Sullivan. Statistical Study of the Growth of Children.

(November) H. K. Haeberlin. Sbetetda'q, A Shamanistic Performance of the Puget Sound Salish.

By direction of the Board of Directors, the Society was affiliated with the New York Academy of Sciences. The alternate meetings of the Society for some years have been joint sessions with the Section of Anthropology and Psychology of the Academy. The present arrangement secures for the Society notice of all its meetings in the Bulletin of the Academy.

Respectfully submitted,

MARJORIE A. MALLORY,

Acting Secretary.

The Secretary was instructed to read the Treasurer's report which follows:

TREASURER'S REPORT

Deposit in Manhattan Savings Institution.....	\$2,917.82	
Deposit in Guaranty Trust Company.....	674.88	
Amount owing Society for dues to Jan. 1, 1918.....	82.00	
	<hr/>	
Total Assets.....		\$3,674.70

Receipts

Balance on hand Jan. 1, 1917 (Guaranty Trust Co.).....	\$726.30	
Interest from Guaranty Trust Co.....	15.98	
Balance on hand Jan. 1, 1917 (Manhattan Savings Institution).....	2,734.27	
Transfer from Guaranty to Manhattan Savings.....	100.00	
Interest from Manhattan Savings Institution.....	83.55	
From dues collected to Dec. 31, 1917.....	469.12	
	<hr/>	
		\$4,129.22

Disbursements

Printing.....	\$ 18.50	
Transfer from Guaranty Trust to Manhattan Savings.....	100.00	
Memberships in Anthropological Association.....	83.00	
Publications.....	326.52	
Louis Meyer (drawings).....	4.00	
Amer. Mus. Nat. Hist. (attendant).....	4.50	
Balance.....	3,592.70	
	<hr/>	
		\$4,129.22
Balance.....	\$3,592.70	
Unpaid dues.....	82.00	
	<hr/>	
Total Assets.....	\$3,674.70	

Respectfully submitted,

ELSIE CLEWS PARSONS,

Treasurer.

Examined and found correct, Jan. 25, 1918.

A. L. KROEBER,

LESLIE SPIER,

Auditing Committee.

The Secretary was instructed to read the Budget submitted by the committee consisting of the Editor and the Treasurer. The budget follows:

BUDGET FOR 1918, AMERICAN ETHNOLOGICAL SOCIETY

Estimated Revenue:

Balance of current funds.....	\$1,292.70	
Estimated income.....	568.67	
Contribution from Carnegie Institution for vol. VII of Publications.....	900.00	\$2,761.37

Proposed Expenditures:

Current expenses.....	\$ 440.00	
Publications, vol. V and vol. VII, Parts I and II.....	1,855.00	\$2,295.00
Balance.....	466.37	\$2,761.37

The Editor of the Society's Publications reported as follows:

Volume V, Koryak Texts, Waldemar Bogoras is now completed, as is also volume VII, part I, Ojibwa Texts, W. Jones. Volume VII, part 2 is entirely in type, 560 pages are being made up. The Carnegie Institution will pay \$900 toward the expense of this publication.

The Secretary was instructed to read the nominations of the Board of Directors to the Society. It was as follows:

The Board of Directors of the American Ethnological Society presents the following nominations for membership in the Society:

Mr. Douglas C. McMurtrie,

2929 Broadway, New York City.

Mrs. C. H. Sorchan,

267 Madison Ave., New York City.

For fellowship in the Society:

Miss Martha Warren Beckwith,

Furnald Hall, Columbia University, New York City.

Miss Ann E. Thomas,

American Museum of Natural History, N. Y. C.

Dr. Walter L. Hildburgh,

Algonquin Hotel, New York City.

Dr. Leo J. Frachtenberg,

Bureau of American Ethnology, Washington, D. C.

Mr. Theodoor de Booy,

125 Lee Ave., Lincoln Park, N. Y.

Mr. Max Schrabisch,

171 Fair Street, Paterson, N. J.

The Society elected the above unanimously.

The Secretary next read the report of the nominating committee, which was as follows:

As a nominating committee the Board of Directors propose the following officers for 1918:

President, Clark Wissler,

First Vice-President, A. L. Kroeber,

Second Vice-President, M. H. Saville,

Secretary, Robert H. Lowie,

Treasurer, Elsie Clews Parsons,

Editor, Franz Boas,

Board of Directors (in addition to the above), P. E. Goddard, A. A. Goldenweiser, N. C. Nelson.

Following the business session Dr. Clark Wissler presented a paper on Cultural Problems of the Southwest. The discussion opened with a brief review of conditions in New Mexico and Arizona in 1600, a resumé of past anthropological research in that region, and a historical sketch of the plan and scope of investigations by the American Museum of Natural History under a grant from Archer M. Huntington. This was followed by an outline of the problems so far developed in the course of the Huntington explorations. The center of the historic pueblo culture can be definitely located in the upper Rio Grande valley, New Mexico. It has been assumed that the many ruins now surrounding the surviving native villages were occupied by the ancestors of these same natives. The archaeological work of the Huntington Exploration makes clear that these ruins were built by people having the same culture as the historic Pueblos, whatever may have been their relation by blood. But the most important result attained was the discovery of stratified deposits giving a chronological scale for these ruins. In like manner research around Zuñi in the Little Colorado drainage gives an analogous chronological scale. In addition to these investigations chronological results were sought in the prehistoric San Juan drainage. The most pressing problem now is to coordinate these three chronological scales so as to form one sequence for the Southwest as a whole. As the case now stands, the ruins of the San Juan seem to have immediately preceded the earliest period of Pueblo activity in the Rio Grande valley.

The next point considered was the association of the living natives with this chronology. The nomadic peoples of the Southwest present the greatest problem here. Their language being Athapaskan, it is a fair assumption that they came from the north. Their culture seems to indicate a Plateau origin from which area they may have been pushed

out by the Shoshoni. On the other hand, the Pueblos themselves seem to have taken their cultural inspiration from the prehistoric San Juan and here again we find an antecedent Plateau culture. It is, therefore, difficult to say who came into the Southwest first, the Athapascans or the Pueblos; perhaps they were simultaneous arrivals.

The paper was discussed by Drs. Kroeber, Parsons, and Boas.

Meeting of February 25, 1918

The Society met at the American Museum of Natural History at 8.15, President Wissler in the chair.

Mr. N. C. Nelson read a paper on the Archaeology of the Southwest. The paper treated the native cultures localized and still surviving in the Southwest as one complete whole and began with the assertion that the order of development to be assumed from the numerous ruins, etc., must be worked out by methods wholly independent of history and tradition. The desired end, it was stated, would be accomplished ultimately by intensive excavations in all of the chief subcenters like the Mesa Verde, Chaco Canyon, and the Rio Grande valley, and by a rigid comparative analysis of the data thus obtained. Some of this work has already been done, but its completion is a long and expensive undertaking. Meanwhile, rather than wait indefinitely for a comparative analysis of all the outstanding culture traits, the American Museum expeditions during the last two years have been concerned mainly with the analysis of one trait, viz., ceramics. This trait was selected because it has universal distribution, shows the greatest variability and the data for it can be obtained in most cases by little or no excavation, requiring nothing more than a reasonably thorough reconnaissance.

Two different analytico-mechanical methods of determining pottery sequences were discussed and graphically illustrated. The results as obtained in this way to date clearly prove the black-on-white wares as a whole to antedate most of the polychrome wares, some of which still survive. This fact applied to the vacated and ruined settlements shows that the ancient high centers of development like Pueblo Bonito and the neighboring villages of the Chaco Canyon were abandoned at a very early date, in fact before anything comparable to it had arisen in such places as the Rio Grande valley. The probably migratory movements, or at any rate the main trend of the cultural streams were indicated and discussed in both aspects, historical and geographical.

The main point to the discussion was that there is a marked correspondence between the historical development of the main traits—such

as architecture, the metate, ceramics, etc.—and their geographical distribution, the older forms having the widest range. That, in other words, the surviving culture complex which we call Pueblo fades away gradually as we trace it back through time in just the same way as it fades away or becomes modified if we trace it out through space. This was taken to mean that the Pueblo culture, so called, was developed in place and that in fact it rests on another culture which is not Pueblo in the commonly accepted sense of the word.

The paper was discussed by Prof. Kroeber, Mr. Spier, Dr. Goddard, and others.

Meeting of March 25, 1918

The Society met with the Section of Anthropology and Psychology of the New York Academy of Sciences at the American Museum of Natural History, President Wissler presiding. Mr. Earl H. Morris presented a paper on The Excavation of the Aztec Ruin, New Mexico.

The first recorded visit of a white man to the Aztec Ruin was that of I. Newberry, August 4, 1859. Morgan described the ruin, and gave a plan of it, in "Houses and House-Life of the American Aborigines," published in 1882. Since the latter 80's, the ruin has been privately owned, and protected from vandalism.

In the spring of 1916, the American Museum acquired the right of excavation, and since then its expeditions have spent seven months removing debris from a portion of the structure.

The ruin is approximately rectangular, 354 x 286 feet, built about a court, with longer axis east and west. The entire area has been cleared of vegetation, and a portion of the south wing, all of the east wing, and parts of the north wing have been excavated.

Cobblestones and adobe were used in the construction of the south wing. These walls are built above the remains of an older structure. The masonry is carelessly done. A section of the east wing constitutes a unit which was built some time after the construction of the main building. The sandstone is of different quality from that of the adjacent chambers, and the floor level is considerably higher, the fill beneath consisting of refuse material. The construction is relatively poor.

The main east wing and the portion of the north wing that has been cleared are part of the original plan of construction. Throughout the masonry is excellent.

Much refuse had been dumped into the rooms of the south wing, whereas the filling of the unit portion of the east wing was due to the decay of the higher portions of the walls. The east wing, excepting two burial chambers, had also been used as a dumping place for refuse.

There is much evidence of successive decay and rehabilitation. Walls that had fallen were rebuilt by the aborigines, and in places walls and rooms were constructed upon the mound resulting from the weathering down of the former structure.

Although in general the walls were well preserved, there were many points of weakness and partial failure. In such cases, the loosened masonry was removed and the walls rebuilt so that what remains of the original structure will stand for many years.

From burial chambers and refuse deposits an excellent collection of pottery, textiles, bone and stone implements, beads, and ornaments, were collected.

Three periods may be distinguished in the chronology of the Aztec region, based upon architectural and ceramic types. The Aztec ruin is characteristic of the most recent of these, and marks the richest development of material culture attained by the ancient peoples of the Upper San Juan drainage. The Mesa Verde cliff dwellings, Pueblo Bonito, certain ruins of the Zuñi region, of the Little Colorado, and of Tularosa valley, were approximately contemporaneous.

Meeting of April 22, 1918

The Society met with the New York Academy of Sciences with Ernest E. Smith, President of the Academy, in the chair. Dr. A. L. Kroeber spoke on Factors Controlling Human Behavior as Illustrated by the Natives of the Southwestern United States. The address is published in the May number of the *American Museum Journal*, 1918.

MARJORIE A. MALLORY,
Acting Secretary

PROCEEDINGS OF THE ANTHROPOLOGICAL SOCIETY OF WASHINGTON

Meeting of March 12, 1918

THE 523d meeting of the Society was held in the West Study Room of the Public Library on Tuesday evening, March 12, 1918, at 8 p. m. The speaker of the evening was Mr. Edward T. Williams, Chief of the Division of Far Eastern Affairs, Department of State, who presented a paper on "The Origins of the Chinese."

Mr. Williams outlined four theories regarding the origin of the Chinese that deserve examination.

The first, advocated by Dr. L. Wieger, a missionary of the Society of Jesus, is that they originated in the Indo-Chinese Peninsula. His reasons for so believing are, briefly, that

1. The Chinese ideograms have existed since 3,000 B.C. and the most ancient represent tropical animals and plants, thus pointing to a tropical country as the place of origin for the race.

But the oldest Chinese ideograms known to the world are not older than 1,200 B.C., when the Chinese were already settled in the valley of the Yellow river and in constant intercourse with their neighbors to the south. These ancient ideograms, moreover, represent animals and plants of the temperate zone rather than of the tropics. Those for sheep and cattle are found, too, in many root words, indicating that the early Chinese were shepherds and herdsmen, pursuits not found in tropical countries.

2. Other reasons given for a tropical origin are that the oldest form of the Chinese language is found in southern China to-day, and

3. The Chinese language is purest in the south and grows more and more corrupt as one approaches the north.

4. The Chinese language is tonal, as are the languages of Indo-China, and is therefore most nearly related to these.

It is not necessary, however, to assume a southern origin for the race to account for these facts, which are just as easily explained by the arrival of the Chinese from the north in successive waves of migration, the later comers crowding the earlier further and further towards the south, so that the oldest and purest forms of Chinese would be found just where they are. The tonal languages of the Indo-Chinese peninsula

in that case are to be regarded as the languages of the vanguard of the migration.

As a matter of history it is now known that many tribes of Cambodia, Siam and Burmah came from the north, the Tibeto-Burmans from a region as far north as the Tien Shan. Some social or physical change forced these tribes to migrate. The dominant element in the population of Burmah did not reach that land until about two or three thousand years ago, while the tribes of Cambodia arrived in their present habitat about 215 B.C. and the Shans, progenitors of the Siamese, ruled southern China until the thirteenth century of the Christian Era. The movement of races therefore has been from north to south and not vice versa.

The second theory is that the Chinese originated on the American Continent. This theory does not require much attention. There have been movements of population, it is true, from America to Siberia, even in historical times, and there is cultural and physical similarity if not identity of the peoples on the opposite shores of the northern Pacific. But the tribes of which this is true lie to the northeast of China and differ strikingly from the Chinese in physical appearance, language and social institutions.

The third theory is held by a number of distinguished scholars and declares that the Chinese are autochthonous and their civilization indigenous. It must be admitted that the oldest existing records of China seem to know no other region as the home of the Chinese forefathers than the valley of the Yellow river, and it is held accordingly that they gave up nomadic habits and settled as agriculturists there in an unknown antiquity and that it was there that they developed their civilization, including their written language. As to the last-mentioned theory is almost certainly wrong. This civilization, including the use of the ideograms, appears to have been shared by surrounding tribes, from among whom in fact some of their most famous rulers came.

One of these tribes, the Chou, headed a league of nine tribes from the west which subdued the Shang Dynasty about 1,200 B.C. These tribes were amalgamated with the earlier, and much of the culture of China must be ascribed to the Chou. This fact and the enforced migration of the Mon-Khmer, Tibeto-Burmans and Shans to the south because of some disturbance apparently in central Asia, give plausibility to the fourth theory.

This theory would place the origin of the race in central or in western Asia. A number of distinguished scholars have held this view. Pumpelly's explorations in central Asia have shown that that region was

the seat of an ancient civilization as old as 8250 B.C. Great climatic changes have there converted what was once a moist and fertile land into an arid desert and caused the inhabitants to migrate to other parts of the world. It was this perhaps that drove the Sumerians into the Euphrates valley and that forced other peoples down upon the Tibeto-Burmans and caused the movements of population in China. The earliest Sumerian monuments show that people to have been Turanian, not Semitic, and to have had obliquely-set eyes. Dr. C. J. Ball, of Oxford, has shown that there are striking resemblances between the earliest Sumerian ideograms and those of the Chinese. He has also published a vocabulary of more than a thousand words which show similarities of sound and meaning in Chinese and Sumerian. This lends weight to the theory that both have a common origin and that the peoples were probably related. Most of the mounds of central Asia remain to be explored and it is not too much to hope that, in the not far distant future, evidence may be found establishing conclusively that the Chinese race originated in that locality.

In the discussion which followed the paper Dr. Aleš Hrdlička called especial attention to the importance of the whole subject and the urgent need of archeological and anthropological investigations on these regions. Others who discussed the paper were Dr. John R. Swanton, Mr. James Mooney and Mr. Henry Farquhar.

Meeting of March 26, 1918

THE 524th meeting of the Society was held in the West Study Room of the Public Library on Tuesday evening, March 26, 1918, at 8 p. m. A paper on "The Origins of the Italian People," especially prepared for the Anthropological Society of Washington by Dr. V. Giuffrida-Ruggeri, Professor of Anthropology, University of Naples, was presented by Dr. Austin H. Clark, U. S. National Museum.

The author leaves aside all that relates to the paleolithic age, in the remains of which Italy is less favored than other regions of western Europe. A more solid ground is encountered in the neolithic epoch. From the Lombard plains to the Ionic shore of Italy archeologists have found repeatedly circular foundations of huts half buried in the earth, the remains of dwellings of a neolithic pastoral people. The huts were hollowed in the ground on purpose, perhaps to afford shelter from the wind, and they were entered either by means of steps, or an inclined plane, or a shaft made close to the hut. In the hollows that remain are found weapons of polished stone and various remains of domestic

handicraft, including pottery of advanced technique, form and decoration.

After describing the burials in natural and artificial caves, the author notes the coming of a new people into Italy from the east. These people came in canoes, and having crossed the Mediterranean they landed on the southern shores of the Italian peninsula as well as in Sicily and Sardinia. They are called Ligures (Liguri) by historians. The Siculi belonged to the same race as the Ligures, and both were physically of the Mediterranean type.

In western Sicily are found similarities to the Iberian civilization, attributable to "that great wave of influence which touched the coast districts of western Europe, bringing with it the dolmen and the dolmen-pottery." The evolution of the *domus de janas* in Sardinia reached its highest development about 2000-1500 B.C. These burials belong to the "eneolithic" age in which copper was used as well as stone. Whilst the civilization of the dolmen and megalithic monuments flourished in Western Europe and in the Mediterranean region there was a different civilization in Central Europe. There we find evidences of a people who lived in the lake-regions on pile-structures (*palafitte*), a people whose history is written only in the refuse of their daily lives, covered today by water and peat-bogs. This refuse shows us a primitive pottery, the cultivation of flax and grain, and a pastoral life. "Toward the end of the second millenium B.C. there took place a great movement of peoples into Italy from the north, and the pile-dwellings of eastern Lombardy, as well as the hut-dwellings of the Ligures, were deserted by their inhabitants." Later the Umbrians and the Etruscans entered Italy.

The question as to who were the "*Italici*" seems superfluous to the author "for there were no special people of that name. Italy is a historic formation and all the antecedent races who contributed to her making are equally 'Italian.' The population of the 'eternal city' was *composita*. It probably embraced from early times the representatives of all the three main races of Europe, the *H. mediterraneus*, *H. alpinus*, and *H. nordicus*."

Meeting of April 9, 1918

THE 525th meeting of the society was held in the Lecture Hall of the Public Library on Tuesday evening, April 9, at 8 o'clock. The speaker was Paul Haupt, Ph.D., LL.D., etc., W. W. Spence, Professor of Semitic Languages and Director of the Oriental Seminary at Johns Hopkins

University, Baltimore, Md. Dr. Haupt's subject was "Mesopotamia and Palestine."

The early civilization of Babylonia was Sumerian. The Sumerian language appears to be related to Georgian in Russian Transcaucasia. Mesopotamia passed successively under the sway of the Sumerians, Accadians, Hittites, Cassites, Assyrians, Chaldeans, Persians, Macedonians, Parthians, Romans, Sassanians, Arabs, Mongols, Tatars and Turks. Since 1638 it has been a part of the Turkish Empire.

In 1902 the Turkish Government granted a German syndicate a charter for the construction of a railway from Constantinople through Asia Minor to Bagdad, and afterwards to Basra. This through line from Hamburg to the Persian Gulf, which threatened the British dominion of India, was one of the most important factors which led to the world war.

In 1886 I recommended colonization of Mesopotamia, construction of the Euphrates railway, and restoration of the ancient system of irrigation. In 1887 I prepared a memorandum concerning a national expedition to Mesopotamia under the auspices of the Smithsonian Institution. In 1892 I suggested settlement of the Russian Jews in Mesopotamia. My plan was afterward advocated by Israël Zangwill (see *The American Hebrew*, May 21, 1909). The restoration of the ancient system of irrigation, which would make Babylonia again the chief granary of the world, was taken up in 1909 by Sir William Willcocks.

The relations between Mesopotamia and Palestine are very close. The ancestors of the Israelites came from Mesopotamia. The Israelites were settled in Palestine when the Edomite ancestors of the Jews were in Egypt. Judah was not a tribe, but a religious association of worshippers of Jahveh, including not only Edomites, but also Horites, Canaanites, Ishmaelites, Moabites, Hittites, Amorites, Philistines, Egyptians and Ethiopians, *i. e.*, a mixture of Asiatic, African, and European elements.

It will perhaps be possible to solve the complicated ethnological problems in Palestine with the help of the new sero-diagnostic methods based on deviation of complement whereby the lytic action of a hemolyzing fluid is prevented. Hansemann made some experiments with Egyptian mummies. Friedenthal tested the blood and flesh of a mammoth which had been found in 1902, imbedded in the ice of Siberia. The reaction showed the near relation of the extinct mammoth to the existing Indian elephant.

Palestine (both Western and Eastern) is nearly as large (9,840 sq. m.) as Sicily (9,860 sq. m.), but it has only about 750,000 inhabitants (Mesopotamia about 1,500,000). Like Sicily, which was the bridge between Europe and Africa, Palestine, the connecting link between Mesopotamia and Egypt, never was the land of a single nation and probably never will be. Certainly the Jews can claim only Judea, not the northern districts, Samaria and Galilee, or the country east of the Jordan. The majority of the colonists whom the Assyrian kings sent to Galilee were Aryans, *i. e.*, Iranians, so that the founders of Christianity may not have been Jews by race.

With the passing away of antisemitism Jewish nationalism will disappear. The Jews in this country will be Americans, the Jews in France will be Frenchmen, but they will continue to regard Jerusalem as their spiritual mother.

Meeting of April 23, 1918

THE 39th annual meeting (526th regular meeting) of the society was held in the West Study Room of the Public Library, April 23 at 8 p. m., President Babcock in the chair. The following officers were elected for the ensuing year: President, Mr. E. T. Williams; Vice-President, Dr. Truman Michelson; Secretary, Mr. Felix Neumann; Treasurer, Mr. J. N. B. Hewitt; Councillors, Mr. J. P. Harrington, Mr. Francis LaFlesche, Rev. John M. Cooper, Dr. E. D. Morgan, Miss Frances Densmore. The society then listened to the address of the retiring president, Mr. W. H. Babcock on "Some Anthropological and National Factors in the Present War."

The speaker reviewed the series of papers on national subjects which had been delivered before the society during the past year.

The war is a contest of nations, conditions and racial aspirations; between the central Teutonic empires, with originally Turanian adherents, and the surrounding republics or liberal monarchies, chiefly Latins, Slavs and the English-speaking peoples. But the difference in kinds of government had less to do with beginning the war than the vehement hostility of races and national ambition.

"Race" and "nation" are variable terms. Language does not always accord with either. No people is homogeneous. What counts for most is a conviction of national identity and racial affiliation sustained emotionally by an ideal of patriotism. When this is violently overridden, a sense of outrage and sacrilege is evoked—the most fruitful source of devastating wars. The best preventative would be such political redistribution as would end alien oppression and make aggression very difficult.

The speaker sketched the human movements which have evolved and defined the peoples of Europe; also the special changes needed. The same victorious powers of civilization which must effect the latter could also maintain them. The prospect of a general and lasting peace was never so good as now; for the world is nearly full and well under control, excepting as yet the central powers and their auxiliaries. There would be no danger from outside barbarians, such as wrecked the long-continued, but territorially restricted, dominion of Rome.

FRANCES DENSMORE, *Secretary*

ANTHROPOLOGICAL NOTES

TITLES OF PAPERS TO BE DISCUSSED AT THE WINTER MEETING OF THE AMERICAN ANTHROPOLOGICAL ASSOCIATION

As the members of the Association have already been informed, in advance notices, it is proposed, at the forthcoming winter meeting of the American Anthropological Association in Baltimore to introduce an innovation by devoting one or two sessions to the presentation of published papers by title and to their discussion. As stated by the President of the Association in his preliminary announcement: "This is not a plan for a series of symposia, but an endeavor to achieve the concrete discussion of concrete papers which has always been regarded as the ideal purpose of meetings. This purpose has failed of realization because the sending of papers so occupies time that only little is left for comment; and also because oral presentation, especially as limited to twenty minutes, is often insufficient to put subject matter before the meeting with sufficiently specific detail to allow of its thorough digestion by the audience. Under the new plan, even technical evidence on which an author's broader conclusions may be based can be assimilated before the meeting, and the discussion therefore conducted with point."

To counteract an erroneous impression which has unfortunately gotten abroad it must be added that it is not intended to curtail,—much less do away with,—the presentation of the results of new researches as under the old plan.

To assure the success of this new venture it is important that the title of the papers to be discussed be communicated to all members of the Association a considerable period in advance in order that they may come prepared to take part; hence, the early appearance of this notice. Following is the final list of papers presented for discussion:

Clark Wissler: Origins, Classification, and Correlation of American Peoples and Cultures. (The American Indian, Chapters XIX-XXI, 1917.)

Edward Sapir: Time Perspective in Aboriginal American Culture. (Canada Geological Survey, Memoir 90, 1916.)

N. C. Nelson: Chronology of the Tano Ruins. (*American Anthropologist*, N. S., XVIII, 159-180.)

- A. A. Goldenweiser: Totemism, an Analytical Study. (*Journal of American Folk-Lore*, Vol. XXIII, 179-293, 1910); Exogamy and Totemism Defined: a Rejoinder. (*American Anthropologist*, XIII, 589-597); The Origin of Totemism. (*American Anthropologist*, XIV, 600-607); Totemism. (New International Encyclopædia); Form and Content in Totemism. (*American Anthropologist*, N. S., XX, 280-295.)
- A. L. Kroeber: The Significance of Kinship Designations. (California Kinship Systems, University of California Publications, in *American Archaeology and Ethnology*, XII, 1917.)
- Frank G. Speck: The Basis of Primitive Algonquian Social Organization. (*American Anthropologist*, N. S., XVII, 289-305; XIX, 9-18; Publications, American Sociological Society, XII, 82-100; Canada Geological Survey, Memoir 70, 1915.)
- Franz Boas: Tsimshian Society. (Tsimshian Mythology, Thirty-first Annual Report, Bureau of American Ethnology, 478-564, 1916.)
- R. H. Lowie: Plains Indian Age-Societies. (*Anthropological Papers*, American Museum of Natural History, Vol. XI, part 13, 1916.)
- Elsie Clews Parsons: Notes on Zuñi. (*Memoirs*, American Anthropological Association, IV, nos. 3, 4, 1917.)
- Truman Michelson: Proofs of Genetic Linguistic Relationship. (Remarks on American Indian Languages, *Journal of the Washington Academy of Sciences*, Vol. VII, no. 8, 222-234, April 19, 1917.)
- Berthold Laufer: The Reindeer and Its Domestication. (*Memoirs*, American Anthropological Association, IV, no. 2, 1917.)
- Louis R. Sullivan: Racial Types in the Philippine Islands. (*Anthropological Papers*, American Museum of Natural History, Vol. XXXII, part 1, 1918.)

DR. W. C. MILLS, the Director of the Ohio State Archaeological Museum, made a careful survey of the famous Flint Ridge District in east-central Ohio. The immediate object of the investigation was to determine the precise nature of the prehistoric flint workings in that vicinity; particularly as to possible chronological relations. Incidentally a classification of the flints is to be made by which it is hoped to trace these materials to other sections of the State and thereby establish lines of culture contact. The season's work consisted in locating the outlying workshop and campsites and a sampling of the accompanying refuse for the classification of materials. In addition a few of the most typical pits were cleared out to determine the mining methods of the aborigines.

Dr. Mills invited the American Museum of Natural History to participate in these surveys and accordingly Dr. Clark Wissler visited Columbus, Ohio, and accompanied the survey party for a part of the season. Later, Dr. Wissler visited the most important mound and village sites in Ohio and began a map of Indiana mounds and earthworks

designed to supplement the map of Ohio recently compiled by Dr. Mills.

MR. LESLIE SPIER of the American Museum of Natural History has recently returned from a field trip to Arizona. The earlier part of the summer was spent on the headwaters of the Salt river where an examination of the archaeological remains was made. Similar work was undertaken farther west on the Verde river. Later a visit was made to the Havasupai among whom Mr. Spier made ethnological studies and secured a collection for the Museum.

DR. A. L. KROEBER spent the month of September at Zuñi where he secured a large body of Zuñi texts and material for a grammar of that language. It is Prof. Kroeber's intention to prepare this long desired material for early publication. Prof. Kroeber is resuming his work in the University of California after six months' exchange with the American Museum of Natural History.

DR. PAUL RADIN has been appointed Instructor in the Department of Anthropology of the University of California.

A DIVISION OF ANTHROPOLOGY has been organized in the Sanitary Corps of the army. Major Charles B. Davenport is in charge; E. W. Hawkes, Louis R. Sullivan, and W. D. Wallis, second lieutenants, compose the division, which was created to secure accurate and systematic measurements and observations in the various training camps.

THE University of Pennsylvania expedition to the hitherto unknown Indian tribes in the mountains between Venezuela and Colombia in charge of Theodore De Booy, curator of the University Museum, has returned, having accomplished its purpose in a much shorter time than was believed possible. This was due largely to the assistance of the Venezuelan government.

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EARLY CHEYENNE VILLAGES

By GEORGE BIRD GRINNELL

INFORMATION as to the region occupied by the Cheyenne in early days is limited and for the most part traditional. Some ethnologists declare that Indian tradition has no historical value, but other students of Indians decline to assent to this dictum. If it is to be accepted we can know little of the Cheyenne until they are found as nomads following the buffalo over the plains. There is, however, a mass of traditionary data which points back to conditions at a much earlier date quite different from these. In primitive times they occupied permanent earth lodges and raised crops of corn, beans, and squashes, on which they largely depended for subsistence.

La Salle says that the Chaa—(?) Sharha—in 1680 told him that they lived about the head of the Great river, and Carver, in 1766, mentions the Schians as found in the great camp of Indians which he visited on the Minnesota river. The Schianese he says live further to the west. Nearly one hundred years later Riggs and Williamson repeat Sioux traditions which declared that in earlier times the Cheyenne had lived on the Minnesota river, but had moved westward.

Two points of permanent occupancy by the Cheyenne seem to be generally accepted; one an earth-lodge village located on the Sheyenne river, a tributary of the Red river from the west—near the present Lisbon, in North Dakota—and the other two neighboring village sites on the Missouri mentioned by Lewis and Clark in 1804, pointed out to them by a Ree chief as having been formerly occupied by the Cheyenne.

The village site near Lisbon (fig. 33) was examined and mapped some years ago by Dr. Libby and Dr. Stout, and a plan (fig. 34) of it is now in the archives of the Historical Society of North Dakota. The two villages mentioned by Lewis and Clark have been roughly located by students of the journals of these explorers.

In the report of the Smithsonian Institution for 1871, Dr. Comfort describes his investigations into certain mounds which he found near Kettle lakes, west of lake Traverse, which he speaks of as made by the Cheyenne.

Disregarding the earliest and very vague traditions of the Cheyenne with regard to their most ancient wanderings and treating Tsís tsís tās and Suhtai as a single group, we find that there still remain in the tribe accounts of a time when they lived on the borders of large lakes in a region which was wholly timbered. This country was presumably in the present state of Minnesota, or to the northeast of that. Later they speak of a blue water, or river, flowing through a "blue earth" country, near which they lived for a long time. This was perhaps the Minnesota river.

Dr. T. S. Williamson¹ records among the "common and most reliable traditions" of the Sioux, one which states that when the ancestors of the Sioux first came to the Falls of St. Anthony the Iowa occupied the country about the mouth of the Minnesota river, and the Cheyenne dwelt higher up on the same river. He states also that the Cheyenne formerly planted on the Minnesota between the Blue Earth and Lac qui Parle.

Writing about 1850, Dr. Riggs² says that "two hundred years ago or thereabouts" the Cheyenne had a village near the Yellow Medicine river in Minnesota, where are yet visible old earth works; that from there they retired to a point between Big Stone lake and lake Traverse, where they had a village, and then moved to the south bend of the Sheyenne river, a tributary of the Red river of the north. This last village is the one near Lisbon, N. D. From the site on the Sheyenne river, the Cheyenne are assumed to have moved toward the Missouri river, and when they reached it are said by

¹ *Minnesota Historical Society Collections*, vol. 1, p. 242.

² S. R. Riggs, *Contributions to North American Ethnology*, vol. ix, p. 193.

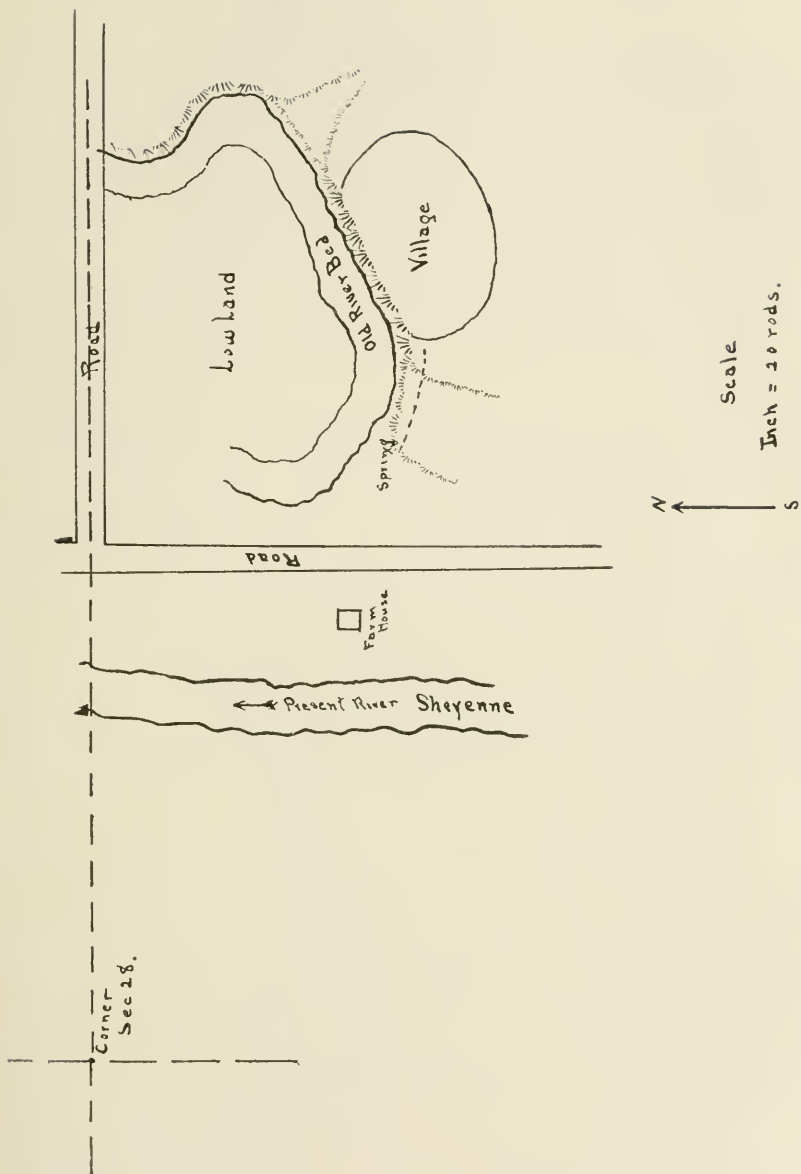


FIG. 33.—Cheyenne Village Site. Described by Geo. F. Will, *Proceedings Mississippi Valley Historical Society*, vol. VII. For further explanation see the following page.

EXPLANATION OF PLOT OF CHEYENNE VILLAGE SITE ON SHEYENNE RIVER—
TRIBUTARY OF RED RIVER

Dr. O. G. Libby, of University, N. D., and Dr. A. B. Stout, of the New York Botanical Garden, who ten years ago examined this old Cheyenne village site on the Sheyenne River, most kindly consent that I should announce the results of their work there; and Dr. Melvin R. Gilmore, Curator of the Historical Society of North Dakota, where the maps and notes on this village site are deposited, agrees that the material should be published. This generous permission enables me to add to this paper the maps made in 1908 by Dr. Libby and Dr. Stout, as well as their recorded notes, which furnish some further details as to the village as they found it. The matter is of interest to students of the Plains tribes, who will be grateful to these gentlemen for the opportunity to learn the results of their inquiry.

The north face of village (fig. 33) is on slope of about 45° to old river bed some 40 feet below. To east a gentle slope extends. A shallow and gently sloping ravine separates village from a round topped broad knoll by road evidently the burial ground.

To the south a gently sloping level area extends. To the west a trail can be traced about 20 rods. It extends down slope to edge of marsh land where a spring is now located.

No large, well defined refuse heaps are to be found. No traces of refuse heaps are to be found in village or along ditch. On the slopes of the bank facing old river channel are evidences of refuse especially near rings Nos. 4-7 and between 14 and 18. At latter place a path leads down to a small refuse pile. A path not only leads directly down but paths come diagonally down slope as shown.

Located on a flat area of land overlooking old river channel and also overlooking surrounding lands to east and south. See larger map (fig. 33) for general topography. Rings inside plowed field—to south of fence indicated—have been plowed over for past 8 years—hence all that could be done was to locate tepee sites.—The centers and ditch showed plainly.

Hut rings in pasture are nearly all quite distinct and outlines are easily determined.

Ditch is from 16 to 28 or 30 feet wide and at opening to west is $2\frac{2}{3}$ feet deep. In no place in pasture is ditch less than $1\frac{3}{4}$ feet deep.

The "cache" pits are shallow depressions. They vary in depth from a few inches to a foot or a foot and a half. Most of them are from three to eight feet across.

No.	1— $32\frac{1}{2} \times 1$
	2— 31×1
	3— $23 \times 1\frac{1}{2}$ somewhat oblong
	4— 28×1
	5— $30\frac{1}{2} \times 2$
	6— 31×1
	7— 26×2
	8— $37 \times \frac{1}{2}$
	9— 38×2
	10— $30 \times 1\frac{1}{4}$
	11— 42×2
	12— $30 \times 1\frac{1}{4}$
	13— $28 \times 1\frac{1}{4}$
	14— $37 \times 1\frac{1}{4}$
	15— $32 \times \frac{3}{4}$
	16— 42×1
	17— $25\frac{1}{2}$ shallow
	18— $36 \times 2\frac{1}{2}$
	19— $17 \times 1\frac{1}{2}$
	20— 38×1

No.	21— $29\frac{1}{2} \times \frac{2}{3}$
	22— $32 \times \frac{3}{4}$
	23— $35 \times 1\frac{1}{2}$
	24— 32×1
	25— $26\frac{1}{2} \times 1$
	26— 30×1
	27— $32 \times 1\frac{0}{2}$
	28— $31 \times 1\frac{1}{2}$
	29— $27 \times 1\frac{1}{4}$
	30— $40 \times 1\frac{1}{2}$
	31— 28×2
	32— 32×1 —irregular
	33— 39×1
	34— $30 \times 1\frac{1}{4}$
	35— $27\frac{1}{2} \times 1\frac{3}{4}$
	36—An irregular shallow depression about 30 feet by 38 feet—with two caches or perhaps a deeper ($1\frac{1}{2}$) oblong depression toward fence.

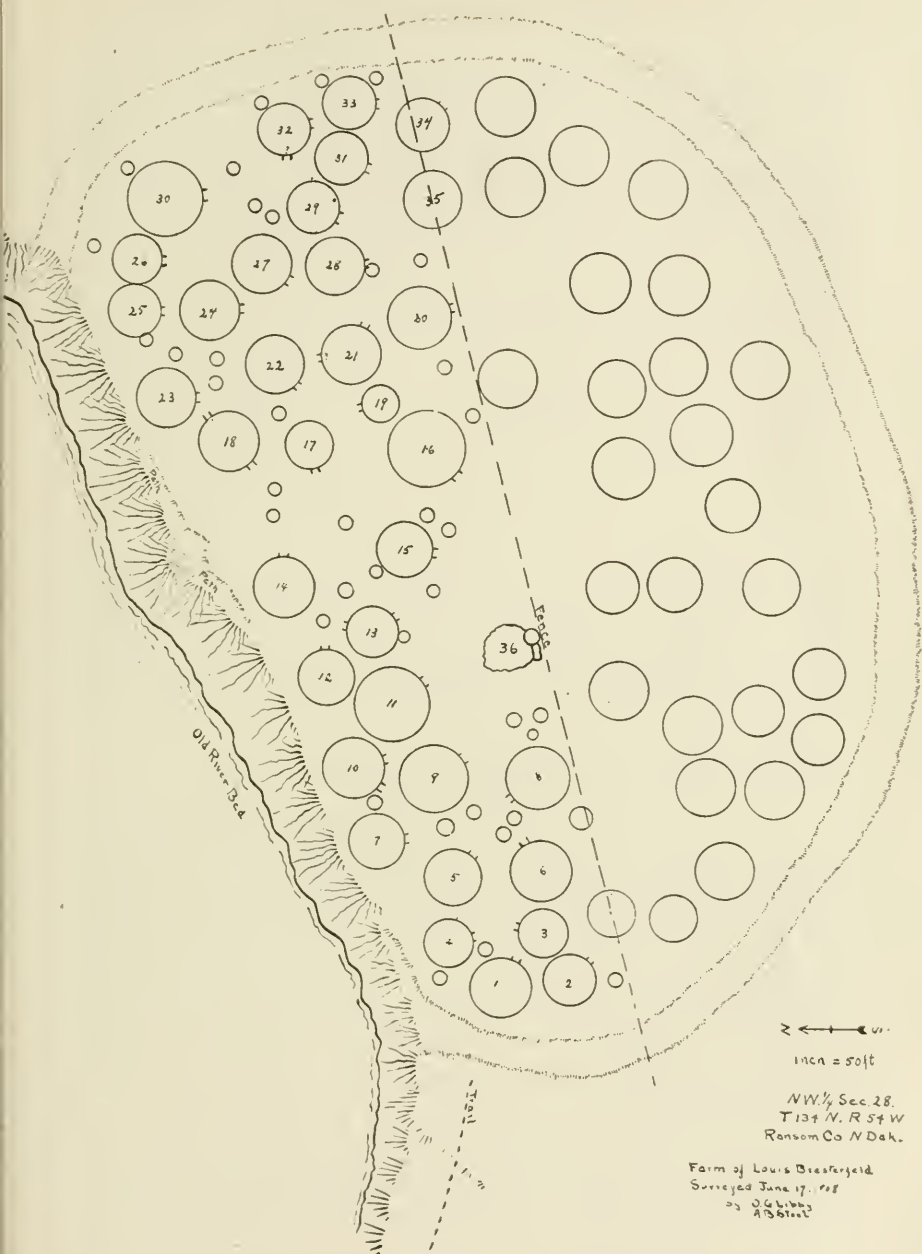


FIG. 34.—Plot of Cheyenne village. Courtesy of Prof. O. G. Libby and Mr. A. B. Stout.

Riggs to have had a village on the east side and afterwards to have crossed the river and to have lived with or near the Mandan.

Dr. Comfort in his account of Indian remains¹—near Fort Wadsworth, west of Sisseton, S. D.—speaks of the occupancy of the region by the Cheyenne as well known, and states that “the Cheyenne about one hundred years ago were dispossessed of the soil by the Dakotas.” The village referred to by Dr. Comfort is not the Lake Traverse village said by Riggs to have been occupied next after that on the Yellow Medicine, but Comfort speaks—p. 398—of mounds and earth works on the shores of lake Traverse which might be the traditional site referred to by Riggs.

Dr. T. S. Williamson² says of the return of the Sioux to the Minnesota river, perhaps many years after their first visit:

The Cheyennes were then in the upper part of the valley; and near the Yellow Medicine a fortification is still plainly visible, which it is said was made by them near a good spring of water, and in 1853, when the first plowing for the Sioux was done in that region, large quantities of muscle shells were turned up near the remains of this fortification, indicating that the ground had been cultivated. The Sioux who expelled the Iowas, a kindred race, made a league with the Cheyennes, who, though of a different origin, have ever since been counted a part of the Dakota nation.

Of the settlement on the Yellow Medicine, Dr. Riggs says:

The excavation extends around three sides of a somewhat irregular square, the fourth being protected by the slope of the hill, which is now covered with timber. After the filling up, of years, or perhaps centuries, the ditch is still about three feet deep. We found the east side, in the middle of the ditch, to measure thirty-eight paces, the south side sixty-two, and the west side fifty. The north side is considerably longer than the south. The area enclosed is not far from half an acre. On each of the three excavated sides there was left a gateway of about two paces.

An early reference to the village on the Sheyenne river appeared in 1863, from the pen of the same author.⁴ He says:

The village stood on the southeast side—of the river—on a high piece of land abutting on a swale which contains springs of living water. More than fifty years have passed since its abandonment by the Cheyennes, but the forti-

¹ *Smithsonian Report* (Wash., 1873), p. 402.

² *Minnesota Historical Society Collections*, vol. III, p. 284.

³ *Minnesota Historical Society Collections*, vol. I, p. 119.

⁴ *St. Paul Daily Press*, August 5, 1863.

fications are all easily traced. The ditch that encircled the village proper is now, in places, three feet deep. It terminates at either end at the bluff blank and is the shape of a half-moon, a little gibbous. It includes between two and three acres of ground. This place was thickly settled with houses. Some sixty or seventy of these houses stood inside the fortifications. Then outside the city were suburban residences, but they were not sunk into the ground nearly so much as those on the inside.

In 1850 the stream on which this village site stands was still called by the Sioux *Sha hī' ē na wo jupi*, Where the Cheyenne plant.

Later Dr. Riggs writes,

Dakota tradition says that it was for a great many years successfully defended by the Cheyennes against the hostile Sioux, that many bloody battles were fought there, the Sioux often being driven back with great slaughter.

In a periodical entitled *The Monthly South Dakotan*, vol. II, no. 4, August 1899, p. 56, is a description of the village by Hon. A. L. Van Osdel of the Sibley Expedition of 1863—the one that Dr. Riggs accompanied. The account possesses a certain interest, but apparently the writer has endeavored to make it vivid by suggesting that the site had been recently abandoned. In fact, in one place he says

Several old dirt lodges were still standing, delapidated and in the lost (sic) stages of decay. These old structures were built in a circular form and were from fifteen to twenty feet in diameter.

Interesting, however, is the fact that this writer has hit upon the truth as to the enemies with whom the Cheyenne fought, and declares that the battles of those who had formerly occupied this place were with the Assiniboine Sioux. This he repeats more than once, and in this he is right, for the fighting of the Cheyenne was with the Assiniboine Sioux, and not with any other Sioux.

The village on the Yellow Medicine was of small extent, and could have been occupied by only a small number of people. One half an acre of land—Dr. Riggs' estimate—would have held but a few lodges. The village on the Sheyenne river was much larger, yet its 60 or 70 lodges, would have hardly housed more than 500 or 600 inhabitants.

Dr. Riggs and Dr. Williamson seem to assume that the different village sites in Minnesota and North Dakota were occupied suc-

cessively, but I believe that this was not the case. It seems more probable that several of the different villages were occupied at the same time and were merely different, scattered, if permanent, camps or villages of different bands of Cheyenne, just as, a few generations ago, different sections of the Pawnee tribe occupied their permanent villages at distances one from another in portions of Kansas and Nebraska.

Although, so far as known, the Cheyenne were never a large tribe, yet as long ago as 1820 Morse¹ gave their number as about thirty-five hundred; and this did not include the Suhtai, then not identified with the Cheyenne. In none of the known settlements of the Cheyenne east of the Missouri river could such a number of people have been accommodated.

I have no doubt that for a long time a number of contemporary Cheyenne villages were scattered along the Minnesota river and to the west of that, and that some of these, after they had been occupied for a generation or two, were abandoned and new locations sought elsewhere. At all events the trend of tribal movement was westward, and this at last brought the Cheyenne to the Missouri river.

In the movement of a group of Indians, a camp or village followed its own ideas as to where it wished to go and did not usually consider the movements of other camps. It moved independently. There was no contemporaneous tribal migration. The different camps did not unite in a forward movement. The trend of the tribal movement being westward, a group moved on, established itself at a point and remained there for a time—perhaps for many years, perhaps for a generation or two. Later, some village behind it moved forward, passed the first village and stopped somewhere beyond. The gradual westward progress consisted of a succession of such movements, the tail of the long procession often becoming the head, and the different camps or villages moving on successively and passing each other. Since for all the people the important question was that of subsistence, it is evident that when

¹ *Report to the Secretary of War of U. S. Indian Affairs* (New Haven, 1822), pp. 251, 254, 366.

a place was found especially favorable for the procuring of food, the camp would remain there longer than it would in a place where the subsistence was less easily had—would be likely to remain in fact until food became difficult to obtain.

The tribal movement westward in fact may almost be compared to the familiar actions of a flock of feeding black birds—or old time wild pigeons—walking over a field in a broad front. The birds in the rear ranks constantly rise on the wing and fly over their fellows to alight just in front of them, where the ground has not been passed over and the food has not been consumed, while the whole front walks forward. In the same way—though slowly—the rearmost camps of the migrating Cheyenne were constantly moving onward and passing those in advance of them in the hope of finding new regions where food might easily be had.

Most early writers who mention the Cheyenne speak of them as having been driven southwest by the Sioux, but, I believe that these statements are due to misunderstanding. Although Lewis and Clark, 1804, Alexander Henry the Younger, 1806, the Rev. Mr. Riggs, 1850, or thereabouts, and W. P. Clark, 1884, all repeat the same story, I am convinced that it is misleading. Long continued inquiry among the Cheyenne reveals no account of any wars with those tribes which we commonly called Sioux, that is the southern branches of the Dakota group. Carver speaks of the Cheyenne as camped in 1766 with the Nadouwessi of the Plains. The western Sioux today declare that they have always been friends of the Cheyenne and Rev. T. S. Williamson says—as stated—that the Cheyenne, have ever since (their first meeting) been acknowledged a part of the Dakota nation. John Hay¹ in his notes on Capt. Mackay's Journal says that the Chayennes or Shayen—who formerly lived on the tributary of the Red river of that name—were so harassed by the Assiniboine and Sious that they had to leave their village and go to the Missouri river.

The Hohē—the Assiniboine—however, are constantly spoken of by the Cheyenne as enemies, and inquiry among the Yankton,

¹ Extracts from Capt. Mackay's Journal and others, *Proceedings of the State Historical Society, Wisconsin* (1915), p. 208.

Hunkpatina, and Teton Sioux now settled on the Missouri river in North Dakota and South Dakota seems to show a general agreement that the Assiniboine were their enemies also, while the Cheyenne were their friends. The Assiniboine used to make war journeys against Sioux and Cheyenne alike. I believe that the Cheyenne tradition of their being driven south by the Hohē refers to early attacks on them by the Assiniboine, perhaps in company with the Cree at first and later with the Ojibwa. That there may have been occasional individual quarrels between Cheyenne and Sioux and between Cheyenne and Mandan is possible and even likely, but, I believe nothing in the nature of a general war.

Dr. A. McG. Beede, of Fort Yates, N. D., wrote me some months ago concerning certain Sioux traditions as to Cheyenne settlements on the Missouri river, and in May last (1918) I went to Fort Yates to make further inquiry into the matter. The Teton Sioux, now allotted and scattered over the Standing Rock Indian reservation, declare that on the west bank of the Missouri river, not far from Fort Yates, there were formerly two Cheyenne villages, and with Dr. Beede I visited the two sites.

The most northerly one is situated on a bluff above the Missouri river on the south side of Porcupine creek, less than five miles north of Ft. Yates. The village has been partly destroyed by the Missouri river, which has undermined the bank and carried away some of the house rings reported to have been well preserved, but a number remain. Of these a few are still seen as the raised borders of considerable earth lodges, the rings about the central hollow being from twelve to fifteen inches above the surrounding soil, and the hollows noticeably deep. In most cases, however, the situation of the house is indicated merely by a slight hollow and especially by the peculiar character of the grass growing on the house site. The eye recognizes the different vegetation, and as soon as the foot is set on the soil within a house site, the difference is felt between that and the ground immediately without the site. The houses nearest both Porcupine creek and the Missouri river stand on the bank immediately above the water, and it is possible that some of those on the Porcupine have been undermined and carried away by that stream when in flood.

This settlement must have been large. It stands on a flat, now bisected by a railroad embankment, slightly sloping toward the river, and the houses stood close together. Many of them were large, one at least being 60 feet in diameter. Besides the large houses there were a great number of smaller ones, probably occupied by small families, by old people living alone, or used as menstrual lodges, or perhaps even for dogs. Including the area east and west of the embankment we counted more than 70 large house sites, taking no account of the small ones. The houses extended several hundred yards back from the river, that is, toward the west, and 150 or 200 yards north and south. It is probable that once they were much more numerous, and they may even have extended a long way down the river, for about two miles below are evidences of another village, said by the Sioux also to have been a Cheyenne village. On the site of this last old village many years ago, a group of Standing Rock Sioux built a number of log houses, the foundations of which have largely obliterated the evidences of the earlier supposed Cheyenne village. This loghouse village was known locally as "Slobtown."

On the gently rising land to the west of the Porcupine village the Cheyenne are said to have planted their corn, as also on the flats on the north side of the Porcupine river. The village site now stands on the farm of Yellow Lodge, a Yankton Sioux, who stated that he had always been told by the old people that this was a Cheyenne village and that in plowing he had often turned up pottery from the ground. Most of this pottery was broken, but he had found some pots that were perfect. He had turned up also glass beads, which he described as like the charms or beads which we know the Cheyennes used to manufacture—in later times perhaps from pounded glass like those said to have been made by the Mandan.

At the time it was impossible to procure any pottery which had been turned up by Yellow Lodge.

Some days later, in company with Dr. Beede, I proceeded to the farm school, less than fifteen miles south of Fort Yates, to examine remains there, also said by the Sioux to be of Cheyenne origin. The

farm school is just over the boundary line between North Dakota and South Dakota, perhaps three miles south of the mouth of Blackfoot creek and a mile below what I suppose to be Eagle Feather creek, and seems to have been established in the very center of this old Indian village. It is east and a little north of the Cheyenne Hills.¹ Just above Blackfoot creek and on the state boundary line is an old village site with three mounds and many house sites said by the Sioux to be Mandan.

To the south and southeast of the school are a dozen or twenty house rings, and to the north, close along the river, are other house rings. Within the boundaries of the farm school are three low mounds. One of these has been excavated to make a root cellar, and by one of the men who had helped dig the cellar I was told that a considerable amount of pottery fragments had been thrown out. On another mound stands the Roman Catholic chapel; and a low mound almost within the school enclosure is partly occupied by one of the office buildings of the school. To the south of the school buildings and on or among the old house rings, are a number of places where modern Indian houses have stood, and small tracts which have been cultivated as gardens within recent years, and are still more or less overgrown with weeds.

The superintendent of the farm school had never heard of a Cheyenne village here, nor of any evidences of Indian occupancy, nor had he seen any pottery fragments. He seemed interested in what we said and walked with us out to a small piece—half an acre—of ground, just south of the school building, the sod of which had been broken only the day before. Here, walking over the newly turned sod I presently found a piece of pottery, which proved to be a part of the rim of a vessel. The fragment is less than two inches wide and two deep. Below the rim extending down perhaps an inch and a half from the unmarked lip of the pot are four lines of ornamentation, parallel to the lip and to each other.

Later several small and unornamented fragments of pottery, two or three flint chips and a few fragments of *Unio* shells, were found.

¹ See *War Dept. Map*, Capts. D. P. Heap and William Ludlow (1875), and Sectional Map of South Dakota, Rand, McNally & Co. (Chicago, 1889).

Alexander Sage, a Mandan Indian about forty years old, employed at the school when shown the ornamented piece of pottery said that it was not Arikara nor Mandan and inferred that it was Cheyenne. Nothing more was seen here. This village was once of considerable size, and the way in which the houses at its border were placed suggests also that attacks by enemies were not anticipated. The cultivation of the soil, the erection of the school buildings, and the westward movement of the Missouri river, which continually undermines the high bank and causes it to drop into the river, have greatly reduced the area of the village.

A few days after this I showed the pieces of pottery picked up at the farm school village to a Northern Cheyenne woman about fifty years old. When she saw them she at once said "my grandmother used to make dishes like that" and described the method of manufacture and of ornamentation by strings of twisted grass—and later sinew—pressed into the soft clay.

I know of no undoubted Cheyenne pottery, yet as recently as fifty years ago, a few Cheyenne women still made clay vessels, though for the most part these had been supplanted by pots and kettles of metal. Among the collections of the American Museum of Natural History there is now a large unornamented earthen pot found eighteen or twenty years ago in the hills back of the Cheyenne village at the mouth of the Porcupine river. This may be a Cheyenne pot though I know of no evidence to connect it with any tribe. When discovered it was spoken of as an Arikara pot, perhaps for no better reason than that the Arikara were at that time the best known makers of pottery along the Missouri river. It is similar to pots known to be made by Mandan, Arikara and other village Indians.

A visit was made to Grand river to look for a Cheyenne village told of by the Sioux as located on that stream about seven miles below the postoffice of Bull Head, and near the camp where Sitting Bull was killed in 1890. The Sioux and Cheyenne name for Grand river is Ree river.¹

Elk River, born about 1814, often spoke of early days when the

¹ *War Map, Dept.* Capts. D. P. Heap and William Ludlow, 1875.

Cheyenne camped on the River of the Rocks, that is, the Cannonball river, and of the time when they lived on the Ree river.

There is still extant among the Cheyenne a song in which Ree river is mentioned. A young girl fond of a boy sings a song asking him if he intends to go to Ree river to marry. It is supposed that the boy was in the habit of leaving his home camp where the girl lived who made the song, to visit the village on Ree river, and she suspected that he was fond of some girl in that village.

There was no difficulty in finding the village site told of by the Sioux on the north side of Grand river, a mile and a half below Sitting Bull's camp. Here were a few house rings within one of which was a hollow—a cache which had fallen in.

A few hundred yards further down the river, on a higher bench, we found many more house sites and in one or two of them the remains of caches. Some of the house sites were forty feet in diameter. They were often overgrown with low bushes. The village was a large one and the house sites ran back nearly half a mile from the river. Sometimes the houses stood close together, and the general plan of the village reminded me much of the old Pawnee village on the Loup river in Nebraska. On a sandy ridge near the river were found a number of large caches, apparently distant from any house sites. Some of these occurred in pairs—two deep holes rather close together. Their situation in the high dry sand seemed well adapted for protecting the stored corn from dampness. These caches were not far from the river bank and on the landward side were protected by the village. Though filled up by the debris of many years they were still quite deep, the surface of the debris being sometimes eighteen inches or two feet below the level of the soil. There must have been room for a great quantity of corn in each of these caches.

According to Sioux tradition some of the lodges here were so large that the Cheyenne took their horses into the lodges at night for protection. I found nothing to suggest the tribe which had occupied the village.

The account of his visit to the Cheyenne camp south of the Hidatsa in 1806 given by Mackenzie¹—although he says nothing as

¹ *Les Bourgeois de la Compagnie du Nord-Ouest*, 1st series (Quebec, 1889), p. 377.

to the direction in which he traveled on his way to that camp—mentions crossing the “Clearwater,” Heart and Cannonball rivers and suggests that the Cheyenne camp of 220 lodges that he visited was on what we know as Grand river. In 1811, the Astorians¹ under Mr. Hunt found on “Big River”—i. e., Grand river—a large camp of Cheyenne.

The great Arikara village at the mouth of Grand river is well known. Whether the Cheyenne occupied this stream before the Ree or after them, or at the same time, is not known. The Cheyenne say—and no doubt this is true of some period—that the Ree were next below them on the Missouri river, and that the two tribes used to live close together, side by side. Some Ree took Cheyenne wives and some Cheyenne took Ree wives. There is much Ree blood in the Cheyenne tribe today.

On our return to Bull Head station we passed the remains of another village where we saw a few house sites. The Sioux speak of this also as a Cheyenne village.

Heavy rains during my whole stay in this neighborhood had rendered the prairie a morass and it was difficult to get about. The whole country here, however, shows evidence of long occupancy by village Indians and is well worthy of further investigation.

Sioux tradition declares that the village on the Porcupine river was established about 1733 or a little earlier, perhaps 1730; they fix the date as about one hundred years before the stars fell, 1833. It was a large village and was occupied for fifty years or more and then the people abandoned it and moved over to a point on Grand river twenty miles above its mouth. The date of the removal is given as about the time of a great flood at this point, which, it is said, took place about 1784. The Cheyenne village remained on Grand river for a long time, probably as late as 1840, for Dr. Beede informs me that Red Hail, a Sioux (born 1833) often told him of visiting the village as a small boy, six or eight years old, and eating green corn there. At this time, however, according to this man's memory, most of the people lived in skin lodges, not in permanent houses.

¹ Washington Irving, *Astoria* (London, 1836), vol. II, 69.

The people who settled on the Porcupine are said by the Sioux to have been the first Cheyenne to reach the Missouri river at that point, though long before this there were Cheyenne west of the Missouri river. The story is that they came from some village in the present Minnesota, described as being on the Minnesota river, near where Mankato now is, where they raised their crops. This account points to them as having lived on the Minnesota river near the mouth of Blue Earth near the old Sioux Crossing—Traverse des Sioux—perhaps in the locality referred to by Williamson and Riggs. At that old village, according to Sioux traditions, there are mounds built by the Cheyenne.

From this Minnesota home, these Cheyenne had journeyed westward, and had passed by the Cheyenne village on the Sheyenne river, which runs into Red river, and gone beyond that to a small flat on the head waters of Maple creek, west and a little south of the present town of Kulm, N. D. At that point, near Kulm, they had built a village and had lived there for a few years. Judge Beede tells me that he has seen there the remains of houses and some small mounds. This village was soon abandoned and they moved on westward, finally reaching and crossing the Missouri river.

Some time after the Cheyenne had established their village on Porcupine creek, still according to Sioux tradition, another group of Cheyenne made their appearance on the Missouri river, crossed into the village of their friends on Porcupine creek, remained there for a time and after no very long stay moved south to a point a short distance south of the present North and South Dakota boundary line, where they established a village a little up river and northeast of the Cheyenne hills. In other words, they established the farm school Cheyenne village, which the Sioux call the Cheyenne Plantings. This group of Cheyenne is said to be the one that long occupied the village on the Sheyenne river, near the present Lisbon, N. D. It is possible that these Sheyenne river Cheyenne may have built and for a time occupied the village two miles below the Porcupine on the site of which "Slobtown," already referred to, was afterward built. They lived at the Cheyenne Plantings, *i. e.*, the

farm school site, for about twenty-five years and then, it is said, moved up Grand river to Dirt Lodge creek, where they built a village of earth houses. This is some distance west of the point on Grand river where the Cheyenne of the Porcupine river located.

The winter count of Blue Thunder, a Sioux historian still alive, records that it was 123 years ago, or in 1795, that the Cheyenne left the farm school village, and moved up to what is now called Dirt Lodge creek. The Sioux say that the Cheyenne village on the tributary of the Red river, near the present Lisbon, had been there for a long time, and the village was very old. It had often been attacked, but the Cheyenne had never been driven from it.

According to Sioux tradition there was a Cheyenne village on the east bank of the Missouri river, opposite the farm school village and there was also a well-known Cheyenne village on the Little Cheyenne river in South Dakota, near the former town of Forest City. This settlement on the Little Cheyenne, referred to by Riggs¹ is one of the many places still known to the Sioux by the name *Sha hī' ēn a wo jū'*. It was occupied for a long time and finally the people are said to have abandoned it and to have moved south.

Old Sioux today talk about the village near Forest City opposite the present Cheyenne River Indian agency—*i. e.*, on the Little Cheyenne river—and of fences, made of sticks set up in the ground criss-cross, and filled in with brush and weeds, which enclosed the corn fields at that place. Of late years, if a Sioux builds a fence where the carelessly set posts lean—are not upright—or are placed too close together the Sioux in derision say it is like the fence about *Sha hī' ēn a wo jū*, referring to this old village on the Little Cheyenne.

Mr. Mooney's statements in the Handbook² as to the farming practices of the Cheyenne seem to indicate that he is unaware, because they are not mentioned in that locality by Lewis and Clark, that they formerly lived on the Little Missouri—Antelope Pit river—and that they grew crops up to the middle of the nineteenth century. But the early Spanish Mss. map brought back by Lewis

¹ *Contributions to North American Ethnology*, vol. IX, p. 194.

² *Handbook of Indian Tribes*, vol. I, p. 251.

and Clark shows a camp of Chaquieno Indians near the head of the Little Missouri.¹

The testimony that they farmed up to 1850 is too general to be ignored. As just stated the Sioux still call a number of old Cheyenne village sites Cheyenne planting places and give various details as to the crops they grew, the way in which they protected them and the time when they moved on further west.

Old Cheyenne women, Wind Women, Ponca Woman, the wife of Brave Wolf, and many others, who were born in or near the Black Hills early in the last century, and who lived on the streams flowing out from them, have many times told me that they commonly planted corn patches, as their mothers before them had done, and had taught them to do.

Accounts of the capturing of eagles as practised from early times down to the first half of the nineteenth century describe as a ceremony connected with this eagle catching the preparation of a certain sort of ceremonial food, which consisted in part of balls of pulverized corn.

The growing of corn is always referred to as a commonplace incident and there is no doubt that it was usually grown. Knowing the conservatism of Indian women, we may feel certain that they would not easily have laid aside the agricultural practices that had come down to them through the generations, but that even after they had moved out on the plains, wherever the situation was favorable, and there was a prospect that they would return during the summer, the old women planted their crops and impressed on their daughters the duty of doing the same thing.

In a recent conversation with Hankering Wolf, who, in 1851, at the time of the Fort Laramie treaty, was a well-grown boy, he incidentally mentioned that, the year before that council was to be held, the Cheyenne put in their crops on a broad flat on the Platte river, just below the main canyon and above the first small canyon above Fort Laramie. It was from this point that they moved down on to the treaty ground at Horse creek.

¹ *Lewis and Clark Original Journals*, Atlas Map 2.

The Cheyenne villages of which we are told by Sioux and Cheyenne tradition and which deserve further study are these:

1. On Minnesota river near Mankato, Minn.
2. On the Yellow Medicine, tributary of the Minnesota river—Williamson and Riggs.
3. On Kettle lakes, N. D. west of Lake Traverse—Comfort.
4. On Sheyenne river near Lisbon, N. D., called by the Sioux, Cheyenne Plantings. Already mapped.
5. On the head of Maple creek near Kulm, N. D.
6. On the east side of Missouri river opposite the farm school.
7. On east side of the Missouri river on the Little Cheyenne river near—former—Forest City, N. D.
8. On west side of Missouri river at junction of Porcupine creek and the Missouri.
9. Two miles below Porcupine creek, possibly a part of Porcupine village, "Slobtown."
10. At Farm School near the Cheyenne hills.
11. On Grand river, near Sitting Bull's camp; and
12. (perhaps) On Dirt Lodge creek, a tributary of Grand river.

Of these, numbers 4 to 11, inclusive, have been called by the Sioux Cheyenne Plantings, *Shā hī' ēn a wo jū'*.

The Cheyenne today tell of villages on the Missouri, at the mouth of White river and of the Cheyenne river.

There seems reason to suppose that the villages just below Porcupine river (8 and 9) were those seen by Lewis and Clark October 15-16, 1804.

The most northerly identified point on the Missouri below these villages is Stone Idol creek, which Coues, Thwaite, and Quaife agree is Spring or Hermaphrodite creek. If we measure off on the Missouri River Commission's map Lewis and Clark distances above the mouth of Hermaphrodite creek, we find that October 13 they camped a mile or more above the former Vanderbilt P. O., on the north side of the river and nearly opposite—a little below—the farm school village. This is a little above the point where the river after flowing east turns south.

The following morning, October 14, the day on which the sentence of the court martial was executed, they left this camp, passed the farm school site—not mentioning Cheyenne ruins or the Chey-

enne hills—which from the farm school site seem to answer very well the description given the next day—October 15—of “curious hills,” like a slant-roofed house. They passed the small creek named, on the Missouri River Commission map Eagle Feather creek, and the larger creek above, the modern Blackfoot creek, which by Coues and Thwaite is considered the Eagle Feather creek of Lewis and Clark. Clark says they camped in a cove of the bank on the north, starboard, side and saw ruins on the south side, which however, were mostly washed into the river. This must have been near the mouth of the stream called Four Mile creek. But we cannot know where the course of the Missouri was at this time nor where Four Mile creek entered it.

The day after this, October 15, during the last three and a half miles of the day's journey, they record, in courses and distances¹ passing a village of the Cheyenne Indians on the south side, below a creek on the same side. The following morning, just after setting out, they passed a circular work where the Cheyenne Indians formerly lived, and just above that saw a creek which they called Chien.

I am satisfied that these two sites are the villages at Porcupine creek, October 16, and at Slobtown, October 15. There is now no running stream just north of Slobtown, though there is a water course which flows in spring. John C. Leach, an old resident, states that in 1872 and in subsequent years this was a running stream which never went dry. Aged Sioux confirm this statement and say that up to twenty years ago the stream carried good water at all seasons and was used by the settlement of Slobtown. The Sioux say that when the old Cheyenne village here was occupied, there was abundance of good water which supplied the whole village and which did not freeze in winter. About sixty years ago a Ree Indian was killed near this creek, and since then the Sioux have called it Paláni wakpála, Ree creek. The name is not found on the maps.

Measurements of the distances between Lewis and Clark's camps on the Missouri River Commission maps bring their camp of October 15 just below the mouth of the Porcupine, but I cannot

¹ *Lewis and Clark Original Journals*, vol. I, p. 195.

locate it. If the Lewis and Clark route is figured back from the mouth of the Cannonball down to the mouth of the Hermaphrodite the distances agree with the Missouri River Commission maps to within two or three miles, as they do when the distances are figured upstream.

The bed of the Missouri river is, of course, constantly changing, and the course of the channel has no doubt greatly altered during the past century.

It seems probable that the last of the Cheyenne left the Missouri river and moved west toward the Black Hills more recently than is generally believed. Perrin du Lac found some of them at the mouth of White river not long before the advent of Lewis and Clark and says that they planted near their village corn and tobacco, which they returned to harvest at the beginning of the autumn.¹

This was precisely the method of the Pawnee. They planted and cultivated their crops in the spring and early summer and then set out on the summer buffalo hunt, from which in early autumn they returned to harvest their crops. It was perhaps mere accident that Lewis and Clark did not come upon an occupied Cheyenne village.

Near the end of the eighteenth century, if we may believe Cheyenne accounts and confirmatory traditions of the Sioux, several Cheyenne and Suhtai villages were still occupied along the great river and its tributaries.

In the year 1877 Little Chief's band of Cheyenne, while being, taken south, was for some time detained at Fort Lincoln, N. D., and among them were the mother of old Elk River (born about 1786) and part of her family. During their stay at Fort Lincoln, this old woman took her daughter and granddaughter about to various points not far from the post, and with laughter and tears showed them the well-known places where, as a girl, she had played and worked. She said that at the time of which she then told, her group of Cheyenne lived in a permanent village on the east bank

¹ Perrin du Lac, *Voyage dans les deux Louisianes*, &c. (Paris and Lyon, 1805), p. 259: "Sement pres de leur village du mais et du tabac, qu'ils viennent recolter au commencement de l'automne."

of the Missouri river and planted there. In the large houses of this village, the grandmother said, there were often a considerable number of people, two or three or four families. She explained that the small house sites in the permanent villages were menstrual lodges, or those occupied by old women who lived alone, as often they did when they were old, and believed that they had not long to live. Elk River was a Suhtai.

White Bull, a Northern Cheyenne, born 1834, declares that in 1832 when High Back Wolf, Limber Lance, and Bull Head went to Washington, the first Cheyenne delegation to visit the seat of the Government, the Cheyenne were still farming on and near the Missouri. It was soon after the death of High Back Bear in 1833 that an increasing number of the Missouri River Cheyenne began to take to a wandering life and to give less attention to farming and some of them to go south. Up to this time many Cheyenne and many Suhtai were planting on the Missouri river. The different camps of Cheyenne, and of Suhtai, were sometimes on one side of the Missouri river and sometimes on the other. On the other hand, Cheyenne and Sioux tradition declares that there were Cheyenne far west of the Missouri river 150 years before that date.

We know comparatively little of the methods and ways of life of this tribe in very early days. No longer ago than the summer of 1918 a Cheyenne woman casually mentioned to me that fifty years ago all Cheyenne women knew how to weave baskets for general uses from a certain grass (*Eleocharis* sp.) and that she herself knew how to weave.

From the many suggested Cheyenne village sites, of which I have written, and from others of whose existence nothing is known, but which I suspect may be found if carefully looked for, there may be recovered archaeological material which may throw much light on the manners and customs of the primitive Cheyenne.

NEW YORK CITY

WAR GOD SHRINES OF LAGUNA AND ZUÑI

By ELSIE CLEWS PARSONS

ABOUT three miles southwest of Suwanee, a water and coal station on the Atcheson, Topeka and Santa Fé railroad, there arises from the plain what might be described as a great ant-hill, an inconspicuous and yet, from its detachment, an easily recognized and remembered spot. (See pl. II, I.) This hill or mound of about fifty feet or more sets about one mile from the mesas to the south and about eight miles from those to the north. About two miles to the east rises a detached mesa the Indians call *mat-saiye*, and to the west stretches a vast plain beyond which south-westerly lies Acoma and northwesterly Laguna. On the northeast horizon rise the Sandia mountains, on the northwest, the San Mateo.

Laguna is about sixteen miles away, and Masseta, an outlying settlement, twelve miles. Suwanee or its neighborhood is called *hanami waha tsiamá* (over there east gate), being actually the eastern boundary of the Laguna reservation, and the shrine we visited inside the aforesaid mound is called *wahaniak shukuk* (east corner) *shtuítawwa* (*shtui* to understand, *tauwa*, good). The shrine is one of the most important of the Laguna shrines. It would be visited by the *cheani* (medicine-men) and *tsatio hucha* (war captains) of Laguna and by the two *kachale* (*cheani*) living at Masseta. It would also be visited, opined my informant, the sister of the *osach* (sun) *cheani*, by men from Acoma, Zuñi¹ and other towns. The old lady herself had never visited the shrine, but it was well known, she said, not only to her but to all the people.

¹ None of the feather-sticks collected was of the Zuñi (or Sia) type, taking the butt of the stick—flat at Zuñi (and Sia), pointed at Laguna (and among the Navajo)—as the most easily distinctive character.

Incidentally, I would note that the butts of feather-sticks in the Museum of the Brooklyn Institute of Arts and Sciences, feather-sticks from *chukuti* shrine, a shrine of the *matkye tsannakwe*, Little Fire fraternity, of Zuñi, are whittled down to almost a point.

Ascending the mound and passing between the low rocks on top you find a sandy terrace from three to four feet encircling an oval opening of about forty-five feet by twenty-five. (See pl. II, 2.) The longer axis runs east and west. Sheer walls of rock girt the pit for about twenty or twenty-five feet and then cut under for ten or fifteen feet. From top to bottom is therefore about thirty-five feet except where, in the middle of the pit, there piles up a hump of soil and rock about five feet high. On this hump there



FIG. 35.—Arrowpoints and shell and turquoise pendants from shrine *wahaniak shukuk shuitanwa*.

is vegetation, including shrubs seven or eight feet high. Elsewhere, around the sides of the pit, there is only the sandy soil from the disintegrated rock. We descended by the thirty-foot ladder which we had brought in sections on the rear of our motor. Because of the cut-under there was no possible way of climbing unaided by ladder or rope in or out of the pit. The pit or mound, Mr. Nelson tells me, is an extinct geyser vent, and there are other like formations in the country.

The soil in the pit, particularly at the west end, had been dug up, presumably by treasure seekers. In 1913 my guide, a white man, had visited the pit together with a rancher whose house stood about a quarter of a mile away from the hill. (The house has since been razed to the ground.) Mr. Eckerman said that at the time of that visit the soil was undisturbed, and that the west end looked



Photographed by Mr. N. C. Nelson.

MOULD WITHIN WHICH IS THE SHRINE WAHANIAT SHUKUK SHTUITAUWA
MOUTH OF THE EXTINGUISHED GEYSER AT THE FOOT OF WHICH IS THE SHRINE

like a big pincushion stuck full of arrows, fired, he thought, from above. They found arrows, he asserted, with stone points attached. He had kept none of these arrows, but the detached stone points as well as the turquoise pendants and the shells he had collected he subsequently gave me. (See fig. 35.)

On my visit, scattered in the upturned soil at the west end,



FIG. 36.—Foreshaft of spear from shrine *wahaniak shukuk shuitua wa*. Three-eighths actual size.

there were a great quantity of worked sticks, hundreds of them, broken and of varying lengths, but with one exception (fig. 36), I found none with points attached, nor did I find any detached points. In this place I found the crook stick shown in fig. 37. The little knob on the stick has been worked and it looks considerably like a duck. The stick is $18\frac{1}{2}$ inches long. It appears to be



FIG. 37.—Crook stick from shrine *wahaniak shukuk shui tau wa*. Two ninths actual size.

quite similar to the crook sticks collected from Bear Creek cave, Blue river, Arizona,¹ or to the crook sticks on Pueblo altars.² Figure 36 appears to be the fore-shaft of a spear. It is $5\frac{1}{16}$ inches long.

¹ Walter Hough. *Culture of the Ancient Pueblos of the Upper Gila River Region, New Mexico and Arizona*, pl. 9, Bulletin 87, Smithsonian Institution (1914).

² A crook stands on one side of the altar of the *osach cheani* of Laguna, and there are crooks on Zuñi altars. The crook used in certain Zuñi ceremonials is thought of as a cane for old age. The crooks (*gnela*) of the Hopi have been described as warrior prayer-sticks, symbols of ancient weapons. On Antelope altars the *gnela* are associated with arrows and in Hopi folk-tales the enemy is overcome by the use of *gnela*. (J. W. Fewkes, "The Winter Solstice Ceremony at Walpi." *American Anthropologist*, vol. XI (1898), 80 n. 2.) On the other hand, Voth describes the crook sticks as symbols of different ages of life. ("The Oraibi Summer Snake Ceremony," p. 3, n. 5. *Field Columbian Mus. Pub.*, 83, *Anthrop. Ser.*, III, No. 4, 1903. Both among the Hopi and at Zuñi a crook stick is used in connection with deceased members of a fraternity—on altars by the Hopi (Fewkes, J. W., "A Few Summer Ceremonials at the Tusayan Pueblos." *Journal American Ethnology and Archaeology*, vol. II (1892), p. 114.) and in feather sticks at Zuñi.

It fits readily into the socket of the stick shown in fig. 38. The sockets in fig. 38, *a*, *b*, *e* are all cone shaped and about one inch deep. In the stick shown in fig. 38 *a* the sinew wrapping has been reinforced by gum.¹

In another place, at the east end of the pit, somewhat to the north, there was another although lesser mass of sticks, and here

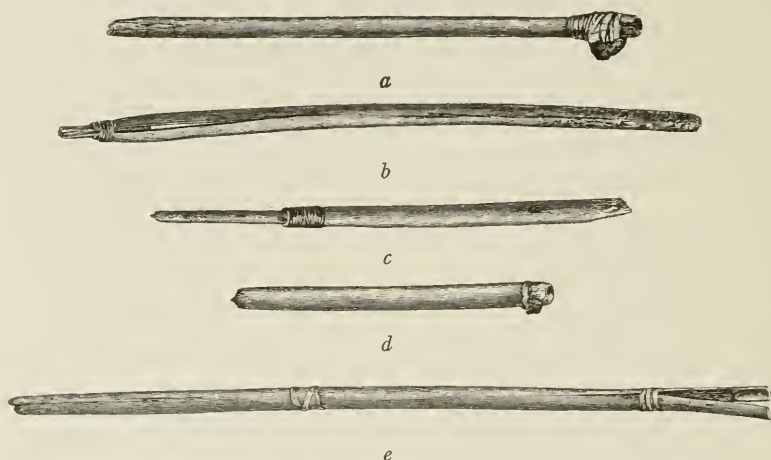


FIG. 38.—*a*, *b*, *c*, *d*, *e*. Sticks from shrine *wahaniak shukuk shuila u wa*. Two-elevenths actual size.

as well as in the other places there were a large number of club-headed sticks and curved sticks similar to those represented in fig. 39.

Sticks similar to the above are used today in Laguna to hunt rabbits.² The boomerang-like rabbit stick of Laguna is without the four grooves which were cut on every boomerang-like stick I noted

¹ Cp. G. H. Pepper, "The Throwing Stick of a Prehistoric People of the Southwest," p. 124, *Proceedings Thirteenth Congress of Americanists*, 1902 (1905). The foreshaft and shafts of the throwing sticks, shown in fig. 36 and 38, are similar to those collected by Kidder and Guernsey from caves of the so-called basket-maker culture in northeastern Arizona and now in the Peabody Museum in Cambridge, Mass.

² A Zuñi to whom I showed the Laguna club said it was a *pilashiwanni* (bow priest) club (*lamkyapnik*, *lame*, club, *kyapnik*, whip). In Zuñi such a club is not used in rabbit hunting. I have little doubt that the clubs in the pit shrine were war clubs although clubs like them are applied today, not to Navajo, but to rabbits.

These sticks are similar to those collected by Kidder and Guernsey in northeastern Arizona.

in the pit shrine. My Laguna informant said the boomerang-like stick was a *hachamuni* (usual word for feather-stick offering) for the rabbits or jack rabbits, and that when offered it was painted and feathers were attached to it. Indentations in the wood and shreds of sinew indicate that feathers may have been attached, but in none of these sticks can I find any sign of paint.¹ None of the sticks I examined, however, looked like a recent deposit.

Scattered throughout the pit were numerous feather-sticks, but those to which feathers were still attached were almost all to be



FIG. 39.—Sticks from shrine *wahaniak shukuk shluitau wa*. One-sixth actual size.

found on or near the central mound. The feather-sticks shown in pl. III, *a, h*, were found hanging in the shrubs, having obviously been thrown down on them from above. The carmine or pink color in pl. III *a* is very fresh. The erect feathers attached to the stick are hawk; the two feathers attached to the netted ring are eagle;² the four feather pendants at the tip are duck, blue bird, and two downy

¹ The Zuñi rabbit stick (*kleane*) is colored black and carmine, the war god's colors. In both Zuñi and Keresan (M. C. Stevenson, "The Sia," p. 44, *Eleventh Annual Report Bureau Ethnology*) myths the rabbit stick is associated with the war gods. In Zuñi war god shrines, however, I noticed no knobbed sticks or rabbit sticks. According to Stevenson, rabbit sticks (and arrowpoints) are offered by the Sia to the Sun. ("The Sia," p. 118.)

² In Navajo tradition a hoop to which two life-feathers, feathers plucked from a living eagle, are attached is associated with the war gods. (Matthews, Washington, "Navaho Legends," p. 109. *Memoirs American Folk-Lore Society*, vol. v. (1897).)

feathers, perhaps turkey and eagle.¹ The stick is $13\frac{5}{8}$ inches long. On the ground to the northeast I collected a similar stick only more faded and with feathers less intact. From the remnants of feathers in both specimens it is evident that a single feather was bound in at the bottom of the stick as in pl. III, *h*. In the stick not figured the binding of this feather was grass. Entirely separate from these netted ring sticks I collected on the central mound or near by five miniature carmine stained bows and five carmine stained arrows, of which one bow and arrow are shown in pl. III, *b, c*. Bow and arrow were not attached. Here too were collected five little sticks of which four are represented in pl. III, *d, e, f, g*. The lower halves of all the sticks are carmine. The zigzag in pl. III, *g*, is painted yellow. In the duplicate of this stick the zigzag is carmine. The sides of pl. III, *f*, are yellow and in the cross design there is a trace of turquoise. Sticks pl. III, *d* and *e*, are unbarked in the lower part except where, in pl. III, *d*, the design is cut. The top of pl. III, *d*, is yellow. An angular design on pl. III, *e* is painted not incised, painted in yellow and black. The longest stick is $2^{13}/_{16}$ inches, the shortest, $2\frac{1}{2}$ inches. Can these sticks be miniature representations of such blunt darts as have been found in caves and dwellings in Utah?²

Four other sticks were collected similar to pl. III, *f* but more weathered. All are painted yellow and black, the dividing line occurring in different places, either below the first pair of feathers as in pl. III, *h* or above, or in two cases just at the string binding these feathers. In two cases the lowest binding is grass.³ As in pl. III, *a*, the feathers are hawk and in the pendant at the tip there are four feathers.

Besides these twiglike offerings, a considerable number of feather-sticks were collected of which those represented in figs. 40, 41 are

¹ Cp. the ceremonial arrow figured by Lumholz in "Symbolism of the Huichol Indians," p. 94. *Memoirs American Museum of Natural History*, vol. III (1900). Cp. also the Hopi *natsi*. (G. A. Dorsey and H. R. Voth, "The Oraibi Soyal Ceremony," p. 17, pl. vi. Pub. 55, *Field Columbian Museum. Anthropological Series*, vol. III. no. 1, 1901.)

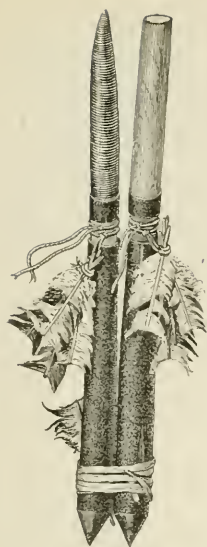
² Pepper, pl. III. The position the sticks were photographed in was accidental.

³ Twiglike feather-sticks were collected from Bear Creek cave. (Hough, figs. 182-188, 191, 194.)

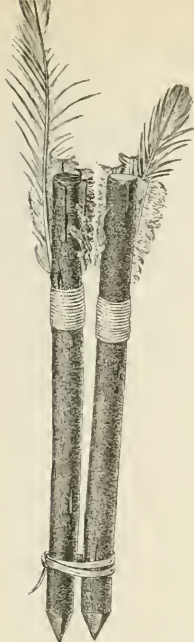


OFFERINGS FROM SHRINE WAHANIAK SHUKUK SHTUITAUWA

A, left, *H*, right, netted ring feather sticks. *B* and *C*, center, miniature bow and arrow. *D*, *E*, *F*, *G*, bottom, left to right, decorated sticks.



a



b



c



d



e



f

FIG. 40.—Feather sticks from shrine *wahaniak shukuk shtuitauwa*: a, 7 in.,
b, $7\frac{1}{16}$ in., c, $7\frac{5}{8}$ in., d, left, e, right, $8\frac{3}{16}$ in., f, $8\frac{1}{8}$ in.

examples. In each case blue or turquoise is indicated in the plate by horizontal lines and yellow, by perpendicular. The darker parts are unbarked and painted black. The rule of contrasting or alternating color noticed by Matthews among the Navajo¹ seems consistently to hold. Although one single stick (fig. 40 *f*) is presented and others were found,² and although several double sticks were found detached, it is not unlikely that originally most of the sticks were in pairs with the characteristic turkey feather at the tie. Taking the side where the turkey feather is inserted as the rear, the right-hand stick is turquoise, the left-hand stick, yellow. In all cases except where feathers may have fallen out there appear to be four feathers. Except for a few turkey and duck feathers the feathers are too weathered for identification. In almost all cases the upper tie is of cotton, the lower of grass. Figure 40, *a*, is identical with sticks made for me in Laguna by the *shiwanna* (thunder) *cheani* and said to be offered during the solstice ceremonials by all the men. Figure 40, *b*, represents *katsena* sticks, being identical with the *katsena* sticks collected by me at Laguna. As in the Laguna *katsena* sticks a turkey feather was doubtlessly inserted at the junction of the sticks. The parti-colored parts of the sticks in fig. 40, *c* are planed with shreds of bark still on the sides.³ At the tip of the stick in fig. 40, *d*, *e*, there are four facets, alternating turquoise and yellow. The shape of this stick is suggestive of the sticks shown in pl. XI, *d*, *e*, and pl. XII, in "The Sia." In noting the yucca ring attached to the stick shown in fig. 41, *a*, *b* we may recall that the Giant (*skoyo*) Society of the Sia attach small yucca rings to their feather-sticks as symbolic, according to Stevenson, of the wheels the cloud spirits ride on.⁴ The stick represented in fig. 41, *e* is undoubtedly ceremonial, but it may not be a feather-stick.

¹ "The Night Chant, a Navaho Ceremony," p. 5. *Memoirs American Museum Natural History*, vol. VI (1902).

² Most of them appear to be solstice ceremonial sticks (see below). There are single sticks in the stick bundle offered during the solstice ceremonial at Laguna.

³ Similar sticks are included in a collection from Laguna in the Museum of the Brooklyn Institute of Arts and Sciences. In this collection there are likewise sticks similar to fig. 40, fig. 41, *d*.

⁴ "The Sia," pp. 91-2. Cp. too pl. XII, *g*.

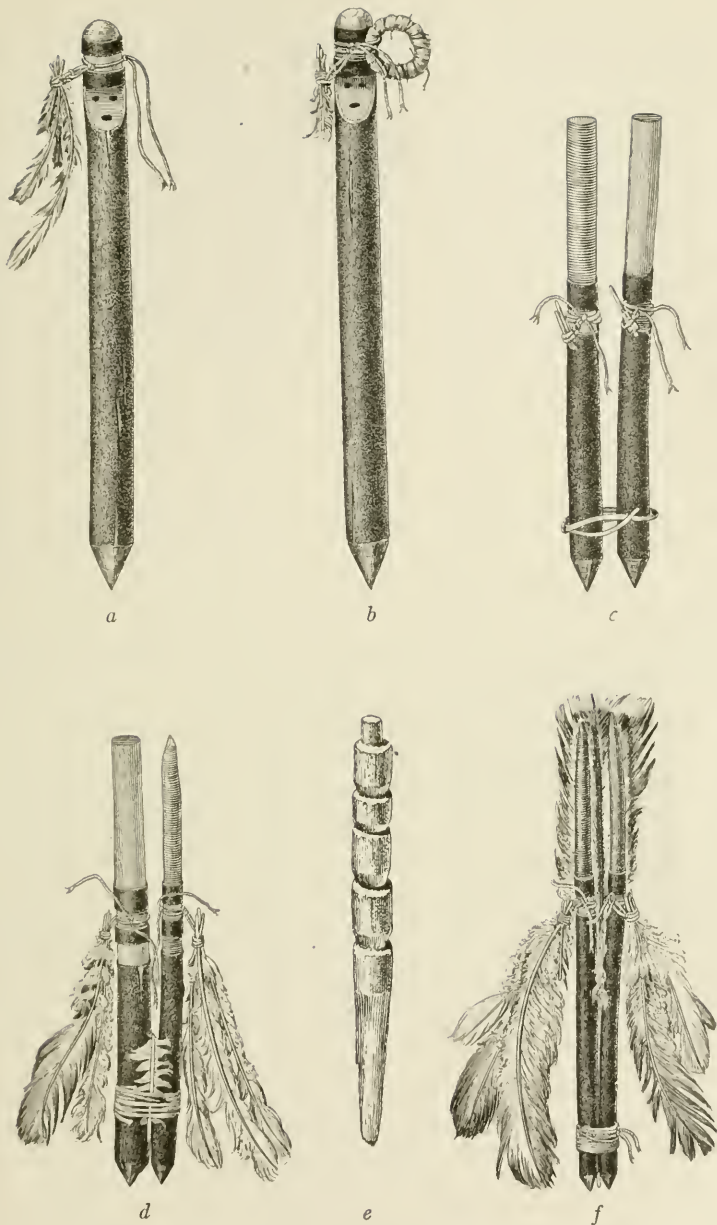


FIG. 41.—Feather-sticks from shrine *wahaniak shukuk shuitauwa*; *a*, left, *b*, right, $8\frac{7}{8}$ in., *c*, $7\frac{1}{8}$ in., *d*, $5\frac{5}{8}$ in., *e*, $7\frac{1}{16}$ in., *f*, $7\frac{1}{4}$ in.

There were in the pit a considerable number, perhaps two dozen or more, snake skins or skeletons and several skulls of small rodents.

From my cursory examination of only a few hours I infer that the main shrine was in the west end, facing, as one might expect, eastward; that formerly spears, clubs, rabbit sticks and feather-sticks, were offered, but nowadays only feather-sticks; and that these feather-sticks are thrown down upon the central mound. Because of the implements of war and hunting found in *wahaniak shukuk shtuitauwa*, as well as the feather-sticks analogous with the war god feather-sticks of Zuñi, I have referred to the shrine as a war god shrine, but, in view of the feather-sticks of other types likewise found, it is possible that the shrine is not exclusively devoted to the war gods.¹

On *towa yallane*, the mesa about three miles to the east of Zuñi, there are four war god shrines, of which one (shrine A)² is long since deserted and two (shrines B and C) neglected in favor of the one (shrine D) which had been comparatively recently laid out. In connection with the annual *lewekwe* and *makye lannakwe* ceremonies four war god shrines are associated with the six directions—(1) north, at *itsanakwi* on top of the mesa about a mile and a half north of Zuñi; (2) west, at *tetlnatluwwayala*,³ a low hill a few hundred yards south of *pinnawa*, a ruin about a mile and a half south west of Zuñi, shrine E; (3) south, on the crown of *ishanantekyapoa tsannakwi* (Little Grease hill), the more southern of the two hills about a quarter of a mile south of Zuñi, shrine F; (4) east, zenith and nadir, on the crowns of two peaks in the foothills at the western base of *towa yallane*, a place called *tonashikwi*, Badger place, shrine G. In addition to these four shrines Stevenson enumerates four war god shrines to the north and, exclusive of those of *towa yallane*,

¹ At Cochiti at the winter solstice ceremonial the *quirana* as well as the orders of the *irshceni*, *shkoio*, and *shikarne* (the curing division of the *quirana*) deposit running sticks, little wheels, little bows, and hunting sticks as an offering to the sun. (Father Dumarest, ms. to be published as a Memoir of the Anthropological Association.)

² I am lettering the shrines which I have hitherto visited and which I am going to describe.

³ A. L. Kroeber, "Zuñi Potsherds," p. 10. *Anthropological Papers, American Museum of Natural History*, vol. XVIII, pt. 1 (1916).

five to the south.¹ The shrines to the north belong, she states, to *uyuyewi*, elder brother war god, the shrines to the south, to *matsailema*, younger brother. "The crater in the volcanic cone at the Zuñi Salt lake" is also referred to by Stevenson as a shrine.² Cushing refers to six war shrines of the war gods, three to the north belonging to *matsailema*, he being associated with the left hand (facing the east, the north is on the left) and three shrines to the south belonging to *ahaiiuta*, he being associated with the right hand. In addition there are two peace shrines, one on *towa yallane* belonging to *matsailema*, and one on the "Mountain of Lovers" (?) belonging to *ahaiiuta*.³

Shrine A is in a cave conspicuously recessed in the west side of *towa yallane*, towards the north and about eight feet from the mesa top. My middle-aged Zuñi guide said that when he was a boy arrows were offered here with points attached.⁴ The *aihayuta*⁵ images

¹ "The Zuñi Indians," p. 606, *Twenty-third Annual Report Bureau American Ethnology*. See, too, p. 481 for the four shrines cited. *Shophlwa yälläkwí* [*shophluwayala*, Spier, L., "Zuñi Potsherds," p. 7. "An outline for a Chronology of Zuñi Ruins," p. 227, *Anthropological Papers American Museum Natural History*, vol. XVIII, pt. III (1917)], a shrine north of the town and near the present school buildings is also mentioned as a war god shrine where in the *lewekwe* ceremonial feather-sticks are deposited ("Zuñi Indians," p. 456). Shortly after this ceremonial I found meal sprinkled here on the loose pit of stones, but no feather-sticks. The proximity of the school may nowadays preclude depositing feather-sticks.

² "The Zuñi Indians," p. 607.

³ *Katalog einer Sammlung von Idolen, Fetischen und priesterlichen Ausstattungsgegenständen der Zuñi- oder Ashiwi-Indianer von Neu Mexiko* (U. S. A.), pp. 1-2. *Veröffentlichungen aus dem königlichen Museum für Völkerkunde*, vol. IV, pt. 1 (1895). In "Manual Concepts," *American Anthropologist*, vol. V (1892), 290-1, Cushing calls *ahaiyuta* the left hand of the sun, and *matsailema*, the right hand.

⁴ According to a *shiwanni* (rain priest), the *apilashiwanni* (bow priests) who are the proxies (*antewa* or *anteliana*, one in place of) of the war gods keep an arrow with its point as a permanent *telanne* (one type of *telikinanne* or feather-stick). One year this *telanne* is kept by *pilashiwanni an papa* (elder brother), and the next year by *pilashiwanni an suwe* (younger brother). The *telanne* is carried by the bow priest when he takes part in the *ikwinnike* or circle dance at the *lewekwe* ceremonial and it is at this time, after the ceremonial, that it passes from one priest to the other.

⁵ *Uyuyewi* and *matsailema* are said by Stevenson to be the war names of the gods and *aihayuta* the peace name they have in common. Referred to either in folktales or in talk I have always heard the war gods called *aihayut* or *aihayut achi* (both); but on asking for their other names I got *masewa* for the elder, and *uyuyuwe* for the younger, the same names given them by the Keresans.

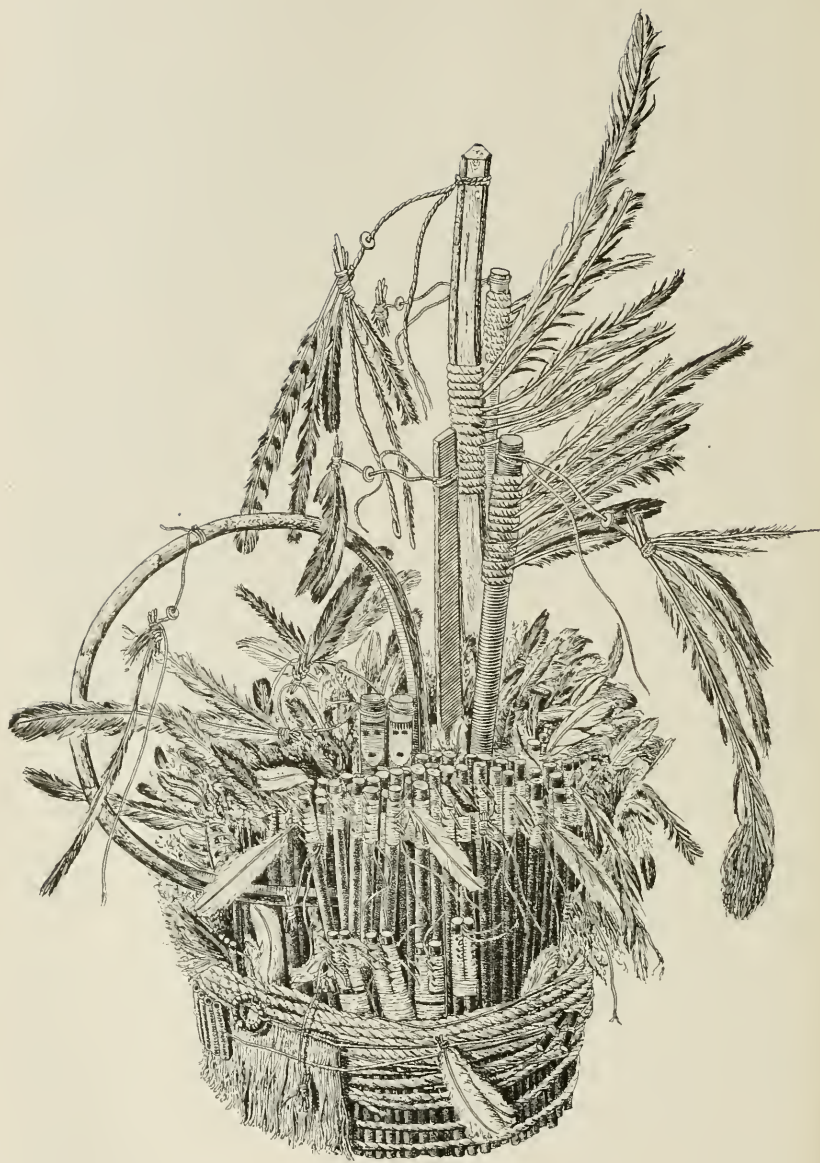


FIG. 42.—*Kya'echine* of *ashiwanni* in war god shrine, *towa yallane*.

that were here have since been removed. In 1881 they were taken out by Mr. Stevenson and photographed.¹ These images are of a considerably cruder type than those of current make. Shrine B is likewise on the western side of *towa yallane*. It is about half way up, placed inconspicuously in a slope of the big pinnacles the Zuñi call *akai'pa*, wide rocks. At the western end of the shrine there stood a much weathered feather-girt wooden image of *aihayuta*

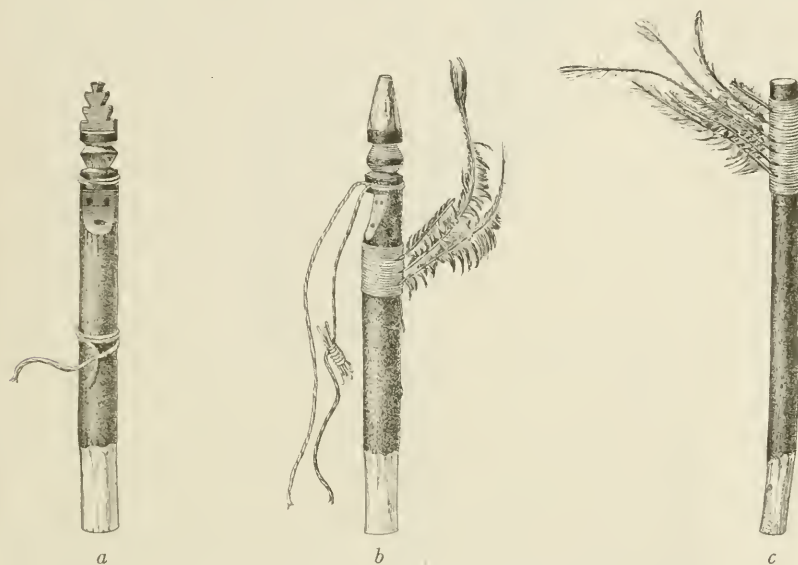


FIG. 43.—Feather-sticks of ashivanni in war god shrine, *towa yallane*.

together with a slat of wood in which the cosmic symbols were cut², and on either side, running east and west, and separated by about three feet was planted a line of feather-sticks, the southern line being as usual that of the *apilashiwanni*, the northern, that of the *ashiwanni*. The *ashiwanni* line consisted of a *kya'echine* or large bundle of sticks (fig. 42) next to the *aihayuta* image and of separate sticks

¹ See "Zuñi Indians," pl. cxxxix. The images were subsequently scattered. One by one they were recovered by Dr. Culin and there are now eleven of them in the Brooklyn Institute Museum.

² See "The Zuñi Indians," pls. xxii, cxxxvii, cxxxviii; Cushing, p. 2. By my guide the slat was called *teshkwin* (altar). Cushing refers to it as an *awilhluia-pon-athlem*.

grouped in sets of three (fig. 43, *a*, *b*, *c*).¹ The *kya'echine* consists of the two pairs of long sticks, the sticks of the *kyakweamosi* (ranking rain priesthood), according to my guide, of a circle stick (*tapone*), of a pair of six and a half inch sticks, one blue, one yellow, of eighty-seven² six and a half inch sticks, and of seven pair of sticks two and an eighth of an inch long. The set of eighty-seven sticks, and the set of the smallest sticks are unbarked and painted black except for the facet on one of each pair of the smallest sticks—it is yellow;³ the sticks in the shorter of the two long pairs are planed and barked and show traces of turquoise paint. The barked under side of the circle of the *tapone* is likewise blue. From one of the longer pair of sticks four strips of bark have been taken off lengthwise but, perhaps because of weathering, there is no trace of paint. My guide, as well as subsequently a *shiwanni*, referred to the *tapone* as *kya'echine awan mosi* (their director). According to the *shiwanni*, the *tapone* is a circle "for the clouds to come down."⁴ The seven major sticks of the *kya'echine* have been wrapped together in a piece of cloth and sprinkled plentifully with bits of white shell, abalone shell and turquoise,⁵ forming as it were an inner case.⁶ On the

The cosmic symbols are associated at Laguna with the war gods through the use of the symbols by the *osach cheani* and the *chakwena* impersonations, *cheani* and impersonations having a war god connection.

¹ Stick *a* and stick *b* are duplicated by sticks collected in another shrine on the west side of *towa yallane*, a phallic shrine for offspring. (See E. C. Parsons, "Zuñi Conception and Pregnancy Beliefs," p. 379. *Proceedings Nineteenth International Congress of Americanists*, 1915).

It is a *shiwanni* who cuts the sticks for this shrine of would-be mothers and from my recollection of the image in the shrine I surmise a war god association.

² It is impossible to be certain of the accuracy of this count without taking the *kyaechin* to pieces.

³ The facet is turned towards the center of the *kya'echine* and so does not show in the picture.

⁴ Cp. the "hoops for the cloud people to ride upon" of the Sia. ("The Sia," p. 74, and the circle *kethawn* of the Navajo, "rings on which the wind-gods ride." ("The Night Chant," p. 67.)

According to Cushing the *tapone* is a throwing stick of the slug type. ("The Arrow," *American Anthropologist*, vol. VIII (1895), pp. 343-4.)

⁵ A seed from this mixture we have been unable to identify.

⁶ To one of the sticks collected at Pueblo Bonito (Cañon de Chelly) and now in the American Museum of Natural History, a stick identified by Mr. Hyde as a feather-stick, a piece of cloth adheres in such a position as to suggest that it was a wrapping for several sticks.

lashowane or feather pendant of each of the seven sticks there is a white shell bead. The tie around the whole bundle is of grass fiber. Outside of this tie and of the fringe of white cotton thread is a cotton thread to which is tied, alternating and at equal distance, two sets of two one-inch black sticks and two sets of two grass-made



FIG. 44.—Feather-stick in war god's shrine, *towa yallane*.

rings. Between each pair two feathers are tied to the common string. A set of four canes is tied on a separate string. The cotton thread around the canes is streaked black on both sides. The feathers in the *kya'echine* are too weathered for adequate identification. Among the feathers are eagle, hawk, jay, duck, flicker, bluebird.¹

At the end of the *apilashiwanni* line of feather-sticks stood the stick represented in fig. 44. According to Cushing, this stick is the god's war club.² Whether this button-shape tip is thus explained

¹ In addition to these feathers I was told by one informant that the oriole (*onolikia*) was associated with the war gods as well as two unidentified birds, the *chaia'a* and the *chewia*.

² *Katalog*, pp. 2-3.

or, when occurring on the war god image or on the stick in the likeness of the image, is explained, again after Cushing, as a fog cap, the resemblance of the tip to feather-sticks from Bear Creek cave should be noted.¹ The stick is $17\frac{5}{8}$ inches long, the measurement being probably, according to information given me in another connection by a *shiwanni*, from the elbow to the tip of the thumb, the regular measure for *aihayut* sticks.² The seven erect feathers are eagle, accipiter hawk, duck, jay, flicker, sparrow-hawk, bluebird; in the *lashowane* the feather of a night hawk takes the place of that of the accipiter hawk. In each of the four *lashowawe* attached to the netted ring there are two feathers, one a blue-bird, the other unidentified. The feathers of the miniature arrow are blue-bird. No paint is left on the stick.³

Shrine C is on the top of the mesa near the northwest edge. The war god image was considerably weathered, although there was still paint on it and to the characteristic projection⁴ a feather was attached. Heaped back of the eastward facing image was a pile of even more weathered images and *teshkwin* pieces. In the discard was a flat oblong piece surmounted by a zigzag piece suggestive of the usual lightning symbol. My guide pointed out this piece as being "the old *aihayut*," meaning, I surmise, pertaining to the elder brother.⁵ At this shrine another man with us said a prayer and scattered meal.

Shrine D was in the southern part of the mesa some distance from the edge, but as our route among the cedars was circuitous I cannot locate it at all exactly. As in the other cases the war god image and the accompanying *teshkwin* faced the east and the two lines of feather-sticks ran east and west. The entire image was

¹ Hough, pl. xx.

² According to one informant the measure is to the tip of the middle finger.

³ I have been told that the *aihayuta* stick (unspecified) is purple (*ky'eakwinna*), purplish corn husk being chewed and spit out to get the pigment.

⁴ Representing, according to Cushing and Stevenson, the navel. Given the appearance of this projection, given the phallic symbols cut in the rocks of the mesa, given the use we noted of what is probably a war god shrine as a phallic shrine, given the role of promiscuity played by the *aihayut achi* in the folk-tales, it seems probable that originally at least another part of the body was represented by the projection.

* However, in native thought today it does represent the navel.

⁵ Cp. Cushing, p. 2.

brilliantly painted and its many feathers were quite fresh, indeed the shrine looked as if it had but just been laid out.¹ Around the neck of the image was a string of tiny olivella shells with an abalone shell pendant.² The ground between the lines of feather-sticks was well strewn with bits of turquoise and white beads and here lay the implements of the war gods' games just as they are pictured in Stevenson's excellent plate of the war gods' altar. The six feather-stick sets of the *apilashiwanni* were connected together by a string as in the plate, the string lying along the ground and parts of the sticks below the string buried in the soil. From the discards behind the shrine I inferred the shrine must have been in use at least ten years. Among the discards I noticed what I was told was last year's set of the game of *iyankolowe*. After a prayer was said at this shrine and meal sprinkled, I was asked how much I would pay for what I might wish to take from it, specifically the set of *iyankolo* pieces. The Zuni is a formalist in his religion—fear of being found out and fear lest something will happen, *i. e.*, magically, is all he appears to know of reverence. Fortunately, from a sentimental point of view, on this occasion there was little call to make a bargain since not only the altar games but an image of the war god were, I knew, accessible for study elsewhere.³

This image (fig. 45) is thirty-three inches long and three and a quarter inches broad. The back and front of the image below the head and creases on either side of the modelled face are stained pink or carmine, the sides of the "body" are blue, the right side of the face is blue, the left side, green.⁴ A cotton tied flicker⁵ feather had

¹ The date of our visit was March 2. Presumably the shrine had been reset at the preceding winter solstice ceremonial, about two months before.

² According to Cushing the olivella shells are the "heart-shells of war" (*isui'ek'-inanne*), the abalone, the symbol of well-being (*Katalog*, p. 2).

³ In the American Museum of Natural History.

⁴ The face of the image in the Berlin Museum is similarly painted, the twofold coloring indicating, according to Cushing, the gods' facility in metamorphosis. Cushing states likewise that not a face but a mask is represented. (*Katalog*, p. 2.)

⁵ According to Cushing, symbolic of the breath of destruction (*sawanik'ia pinnane*). (*Katalog*, p. 2) *Sawanike* is a term used of the yucca switches of the *koko*, of a rabbit stick, of the arrow-point tied on a hunting fetish animal, of the heart. A great deer hunter is described as *na* (deer) *sawani*.

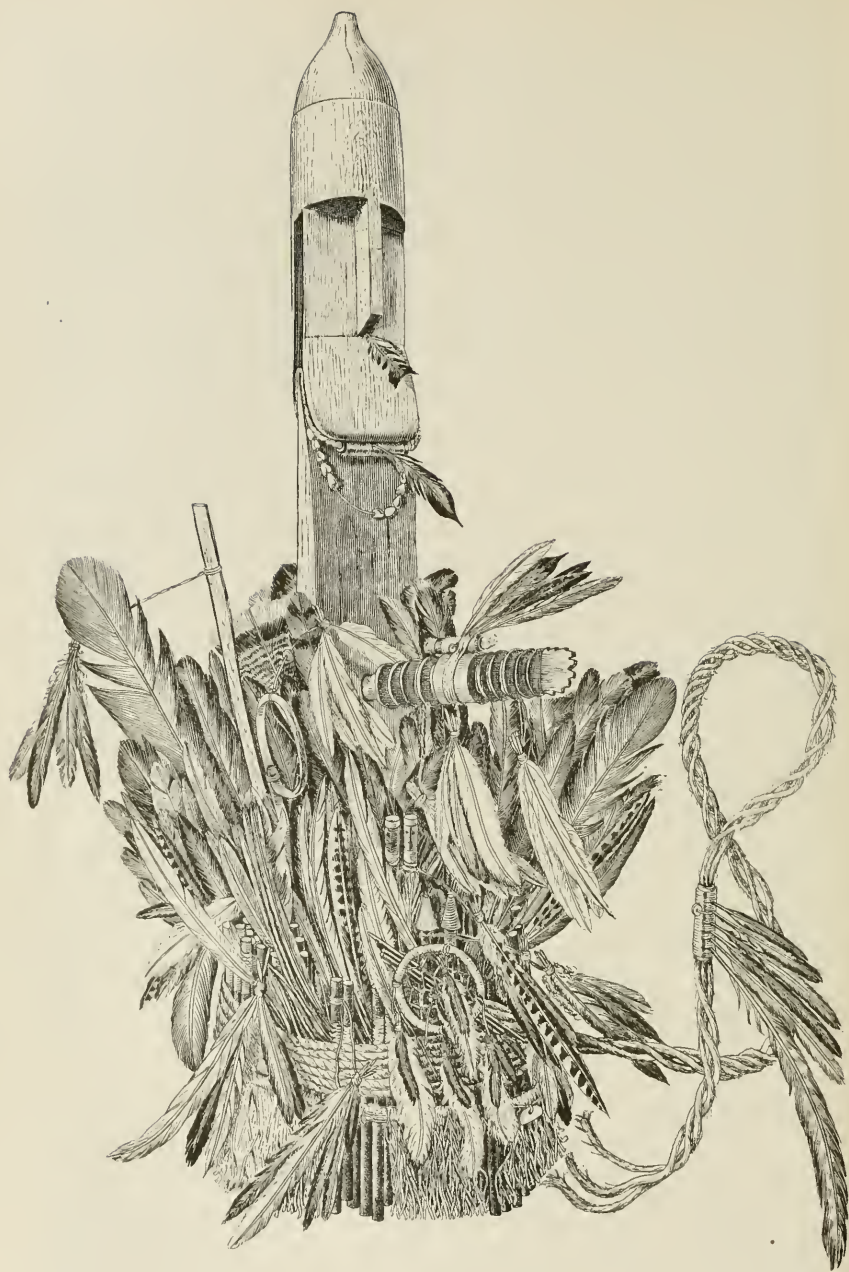


FIG. 45.—War god image and feather-sticks from shrine on *Iowa yallane*.

been inserted into the wood below the nose.³ Another flicker feather is bound into the cotton thread wrapped around the inch and a half of cane through which runs the twist of fiber around the neck. The cotton thread is streaked across with black. The necklace is of olivella shells. The projection from the body is painted turquoise. The five feathers in each of its three *lashowawe* and the erect feathers fastened into the cane bound on top are eagle (?), duck, sparrow-hawk, flicker, jay, blue-bird. A white bead is strung on the cotton thread around the cane. Inside the cane there is a twist of raw cotton. The feather-sticks around the base of the image, a rounded block painted black,² number about fifty. These feather-sticks are grouped by color, so that, with the image facing east, the yellow sticks would be on the north, the blue on the west, the red on the south, and the white on the east, a distinction in accordance with the color and direction symbolism of Zuñi. There are a number of variously distributed black sticks. Some of these sticks are of the same diameter as the colored sticks, others, five of them, are more slender and the tip is tapered. Of the latter sticks, feathers and strings had been painted carmine. The stick is the type represented in fig. 46, *a*, *b*.³ Between the red sticks and the white, *i. e.*, on the southeast there is a group of blue, black and yellow sticks which together with the red on one side and the white on the other probably represent the parti-colored zenith.⁴ In front of the image are two black sticks with turquoise caps. The netted ring,⁵ the bow, the two arrows, and the club fastened to one of the sticks are carmine. Around the lower part of the sticks is tied the fiber twist seen on the right of the picture. This twist is about three feet long. About a foot from the free end it runs

¹ The legend goes that once the god fled into a mole hole from a deer and the deer with his antler pierced the god's nose. Through the hole the god stuck a flicker feather. The well-known myth of the creature that borrowed a passage for the war gods to the heart of a monster is, I take it, referred to.

² Without damaging the bundles it is impossible to see whether hands are painted on the block as is the case with other war god images.

³ Cp. "Zuñi Indians," pp. 596-8.

⁴ Objects of variegated color are placed on the southeast by the Hopi, but variegated color represents the nadir. ("The Oraibi Soyal Ceremony," p. 22)

⁵ Called *piallane*, to come before. A curtain or shield (?) is thus called. The netted shield is *aiuchiana*, magical, nothing would pierce it.

through a cotton thread-wrapped cane like that at the neck of the image. This cotton thread is also streaked with black. On the side of the cane opposite the six feathers a single white bead is held by the cotton thread.

Back of the blue capped sticks are the sticks represented in fig. 46, *a*. Both these sticks are blue, the facet of the left-hand stick being yellow. The modelled facets or faces on the sticks are interesting as a link between modelling on the war god image and the ordinarily flat facet of the feather-stick with its three black dots for eyes and mouth. In their modelling the sticks are like a stick collected from Bear Creek cave, Arizona.¹

To the right of the image are the sticks represented in fig. 46, *b*. The barked inner side of the circle of the circle stick and the two planed sides of the upright stick are painted turquoise. The circle stick is $8\frac{3}{4}$ inches long, its mate, $14\frac{5}{8}$ inches. The five feathers of the circle stick are turkey, eagle, duck, flicker, blue-bird. From the two *lashowane* the duck feather is omitted.

Back of the above sticks is the single stick represented in fig. 46, *c*. Grass² ring as well as stick are painted black. The erect feathers are turkey (?), duck, one unidentified feather, sparrowhawk, flicker, blue-bird, duck (reversed).³ The two feathers in the *lashowane* are turkey.

Among the sticks at the back of the image is a cane cigarette about five inches long, with one end charred, and a similar cane cigarette is among the loose objects collected with the image. I infer the latter has fallen out of the bundle.⁴

¹ Hough, fig. 193. Cp., too, fig. 197.

² A similar ring I have noted in a summer rain dance tied to a knob on the mask of *koyemshi awan tachu*.

³ The last feather towards the proximal end of the stick in the feather stick of the *koko* or masked impersonations is duck and it is reversed. The stick in question is undoubtedly a *koko* stick and, I infer, the stick of *koyemshi awan tacher*. Among the game implements collected with the war god image was a yucca ring used in the pole and hoop game played by the *koyemshi* for rain. ("Zuñi Indians," pp. 345-6.) The small cylinders and rings already noted on the *ashiwanni kyacchine* may be symbols of this game. Cp. Fewkes, J. W., "A Few Summer Ceremonials at the Tusayan Pueblos." *Journal American Ethnology and Archaeology*, vol. II (1892), pp. 131-2, 142-4.

⁴ Cp. "Zuñi Indians," p. 455. Cane cigarettes are fastened to several of the feather-sticks collected from Bear Creek cave (Hough, pl. 18).

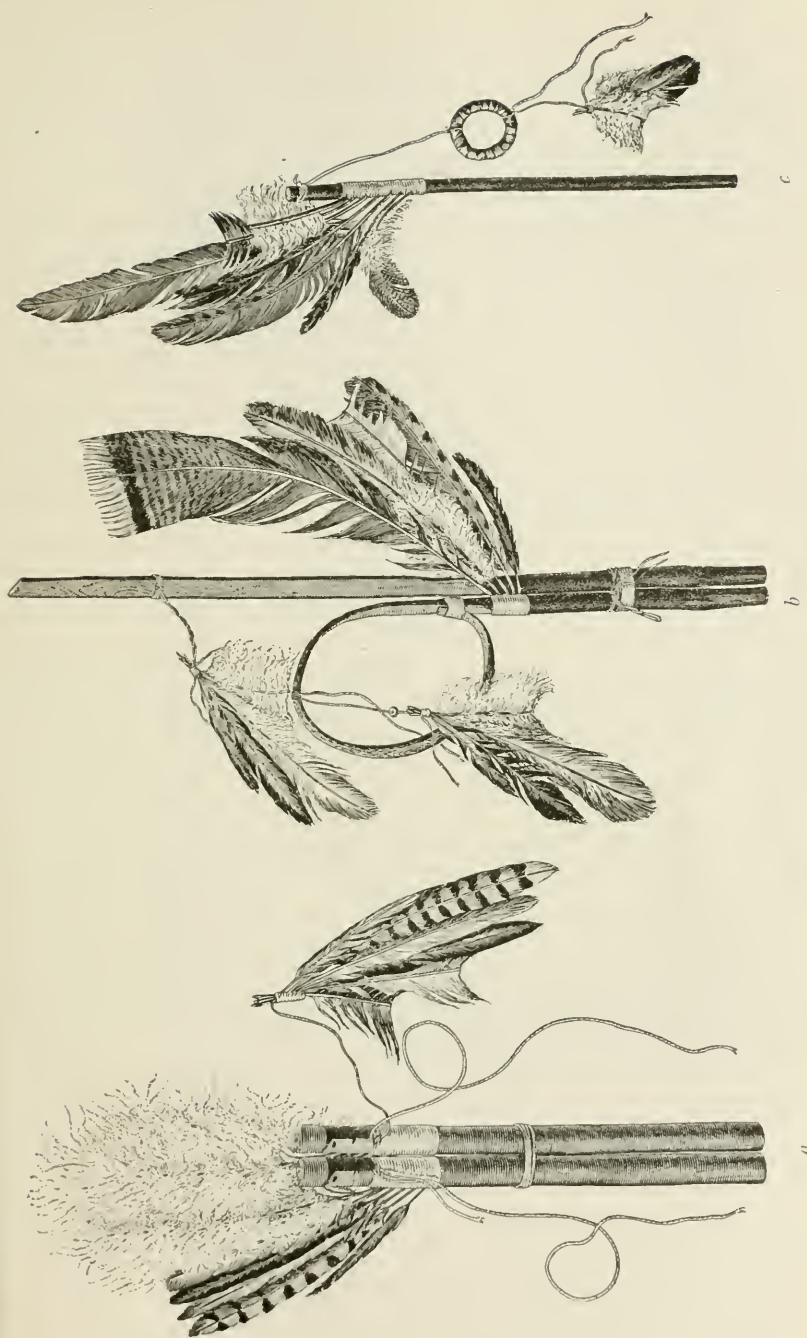


FIG. 46.—Feather-sticks wrapped around war god image.

Among the lower part of the bundle is tied a cotton thread to which on the face side of the image a fringe of thread is attached. Midway, to one of the strands of this fringe, is tied a large flat turquoise bead. This fringe is called *hakwani*.¹ It is made on a frame, and represents the dress for the image.² The work is done during the four days before the beginning of the winter solstice ceremonial. The first night of that ceremonial the images are assembled in the maternal house of the maker of *an papa*. Paper bread, balls of meat, etc., are set out in front of the images, the images are given to eat (*itokyinan*) and then all present eat. Subsequently the images are taken to *he'iwa kwitsine*. Between the feather-sticks and the image there is first a layer of plant fiber and then next to the image a layer of raw cotton. We recall that the *ettowe*³ of the Zuñi are similarly wrapped with cotton. So is the *iärriko* (corn ear fetich) of the Sia,⁴ and around the *iärriko* feather-sticks are set in a way quite similar to those set around the image of *aihayut*.

Shrines E-G, unlike the shrines of *towa yallane*, are all of the stone slab type. I will describe shrine F first as it was first visited and my notes on it are the fullest. A space about two feet and a half square is enclosed with stones and about two feet and a half from the ground covered over with flat stones.⁵ A number of

¹ It is like the fringe already noted on the *ashiwanni kyachine*, and it is like the fringe around the neck of a Hopi mountain lion stone fetish in the American Museum of Natural History.

² It is made by *ainshikwe* (bear) clanswomen and, because there are so few of them, by *ainshikwe awan chawe* (their children) *i. e.*, by women whose fathers are *ainshikwe*, and even by women whose fathers are *ainshikwe an chale*. It is *ainshikwe* or *ainshikwe awan chawe* men who make the image of *aihayuta an suwe*, and the miniature game implements, and *showitakwe* (deer) men who make the image and games of *aihayuta an papa*. Nowadays the two *aihayuta an suwe* image-makers are Zuñi, *ainshikwe an chale*, married into the house of the *ashiwanni* of the South, and La'usi, *ainshikwe*, one of those *ashiwanni*. The younger *ainshikwe* men or boys work on the games. The image-maker of *aihayuta an papa* is the brother of the present *komosona*. The *komosona* himself does not work "because he is the boss of the dancers." The paramount *ashiwanni* and the *apilashiwanni* make the engirdling feather-sticks.

³ Fetiches of corn ears or seed filled canes. Cp. too the *ponepoyanne* of the *apilashiwanni* ("Zuñi Indians," p. 598 n. a.) as well as the *lashowan lanna* of the *apilashiwanni* and the *makye lannakwe*.

⁴ "The Sia," p. 40, n. 1 and pl. ix.

⁵ See "Zuñi Indians," pl. cxiii.



FEATHER STICK FROM WAR-GOD SHRINE AT ISHANAN TRKYAPOI TSANNAKWI, BOTH SIDES SHOWN
NETTED RING, MINIATURE BOW, ARROW AND CLUB FROM WAR GOD TELAN

feather-sticks like those in fig. 46, *a, b* were strewn, *not planted*, inside on the ground.¹ To the five $6\frac{1}{8}$ inch sticks represented in pl. IV, *a, b*, *lashowawe* had been attached. There are three or four erect feathers to each of these tapered sticks—eagle, Cooper's (?) hawk, sparrow-hawk, flicker, duck (white),² robin, blue-bird. Feathers as well as sticks are stained carmine. (Red, we recall, is the color of the south.) On the south side of this enclosure is a pile of stones in which were held erect two staves about five feet long.³ To the end distal of each staff erect feathers were attached as well as a *lashowane*. The proximal end was wrapped with corn husk. Near the middle were bound some erect feathers, a netted ring, and miniature bow, arrow, and club. An olivella shell was attached to the bow and *lashowawe* to the ring. Pl. IV, *c* represents the ring, bow, arrow, and club I detached from one of the six or seven discarded staves scattered on the ground. Two feathers had been in each of the five *lashowawe*. Judging from a like ring collected elsewhere the feathers were blue-bird and probably eagle. The olivella shell is strung on the bow string.⁴ Fastened in with the erect staves was a large bunch of small spruce branches, to the tip of one of which was tied a *lashowane*. On the ground lay a discarded bunch of spruce with a *lashowane* attached. Besides the staves in the discard on the ground were a number of cylindrical carmine stained sticks somewhat less than two feet long and about $\frac{1}{16}$ of an inch in diameter,⁵ and around the bottom a cornhusk was wrapped as it is wrapped around the ends of feather-sticks.⁶ In the dance these sticks are attached to the distal ends of the staves,

¹ According to Stevenson these feather-sticks are put in by the *lewekwe* fraternity during their ceremonials in winter. ("Zuñi Indians," p. 481.)

² Domestic duck. Domestic duck feathers are used in the feather-sticks.

³ According to Stevenson, they are the *telawe* made by the *apilashiwanni* and carried in a rite in connection with the *lewekwe* ceremonial ("Zuñi Indians," pp. 450 n. b., 481), and they are the length of the extended arms.

⁴ A like shell is similarly strung on the miniature bow offered by a Hopi warrior chief in the Soyal ceremonial of 1903 and now in the American Museum of Natural History.

⁵ Cp. the sticks figured in "The Oraibi Soyal Ceremony," pl. VIII.

⁶ There are in the American Museum of Natural History four of these sticks, one from shrine F, three from shrine G. The shortest sticks measure $17\frac{1}{4}$ inches, the longest $19\frac{1}{2}$ inches. All are carmine.

and among the discards I found one of these sticks bound to a staff.¹

In the picture of the shrine E at *telnatlucwayala* (fig. 47) a similar group of staves and shorter sticks is represented. The staves were colored blue. In the two sets of erect feathers on the staff from this shrine in the American Museum of Natural History there are seven feathers, blue-bird, flicker, jay, sparrow-hawk, duck



FIG. 47.—War god shrine at *telnatlucwayala*. Photographed by Mr. Leslie Spier.

and two eagle feathers. In the *lashowane* there are the same feathers lacking one eagle feather.

Shrine E is divided into two compartments. In both were strewn a number of tapered feather-sticks of the type represented in pl. iv. Some of them were painted blue (blue, we recall, is the color of the west) and some were unbarked and painted black. Feathers and even sticks were considerably disintegrated.²

Shrine G at *tonashikwi* appears to be in two parts—staves and short sticks were in a cairn of stones on the highest peak, but the slab shrine is on another peak, the peak furthest to the north. The

¹ Cp. "Zuñi Indians," p. 450 n. b.

² Shrine E and shrine G were visited in September. The offerings in both shrines had been partly destroyed by insects. In shrine G there was a two inch layer of what appeared to be winged ants.

oblong slab shrine is divided into three compartments—the long axis north and south. In the northern compartment was a *kyá'echine* of twelve or more tapered sticks and in the middle compartment another like bundle. The sticks were white (for the east), spotted (yellow, blue, red, white and black, for the zenith), and black (for the nadir). In the southern compartment were six tapered sticks planted in a row. They were unbarked and painted black.

About three feet to the south of this compartment shrine was another slab shrine containing fraternity feather-sticks and about twelve feet to the east was still another slab shrine of fraternity feather-sticks. These sticks will be figured in a later paper.

NEW YORK CITY.

SOME ETHNOLOGICAL AND NATIONAL FACTORS OF THE WAR¹

By WILLIAM H. BABCOCK

THIS year all the world has continued in turmoil, as everyone knows. Whatever else may be said of the war it is surely a stupendous anthropological phenomenon. It has seemed inevitable that the Anthropological Society of Washington should give it first place in attention, at least to the extent of investigating and explaining some of the factors involved, or liable soon to be involved, in the struggle. No better plan suggested itself, or was suggested, than a series of papers on the various peoples, sketching as far as possible the earliest archaeologic and anthropologic data, the racial origins, shiftings and blendings, the historic development and their present or very recent status, social, political and ethnologic.

In view of the small number of meetings in a single season and the brevity of each, it was evident that exhaustiveness was impossible and compromise was necessary. So the lectures have been confined to the old world only and the greater and more complex powers have generally been exhibited as such only in one phase of their history or not at all, their place being taken by lesser but very distinctive national units more susceptible of adequate treatment in the limits available. Of course not all of these minor national entities have been given and for some topics we have relied on papers read before this society in recent past seasons. In spite of restrictions and compelled omissions, since the beginning of our season last October we have been able to present with fair approach to completeness Bohemia, Greece, Belgium, Roumania, Scandinavia, Japan, Poland, Scotland, Russia, the Origins of the Chinese, the Origins of the Italians, Mesopotamia and Palestine—a widely distributed array of distinctive and immediately important peoples.

¹ Address as retiring President delivered before the Anthropological Society of Washington, D.C., April 23, 1918.

The great body of information in these monographs or lectures, with other accessible data, should enable us to judge of the causes and nature of the present conflict and the means of preventing its repetition.

Obviously, the issue is between nations; almost as obviously between diverse conditions and antagonistic racial desires or aspirations. In common parlance it is a war between the central empires and the allied nations of the entente surrounding them, either in contact or at various intervals. The central empires are dominantly Teutonic, supported strenuously by divers more eastward peoples, Hungarian, Bulgarian, Turkish, all originally non-Aryan, but modified by long occupancy in the west. Their enemies are mostly English-speaking or Latin or Slavonic, excepting the peoples of the Far East who have as yet taken little part in the western battles. Again, the enemies surrounding the central empires are nearly all republics, or monarchies of the limited liberal type, which may be considered republics in all but name. According to the line of distinction chosen, it may be justifiably looked on as a war between democracy and autocracy or as a war of the Teutons and their partly Turanian adjuncts against the Slavs on the east and southeast, and Latins of the west and southwest, the latter being reinforced by the English-speaking peoples and certain oriental countries. It is to be noted, however, that a difference in kinds of government had much less to do with the beginning of the strife than the vehement hostility of races, and national ambition.

Of course such terms as race and nation are fluid and uncertain in ordinary use. Most broadly applied, the former covers the whole human race; a little less broadly, some of the great divisions, as the Caucasian, Mongolian or Negro race; more narrowly, we distinguish the Semitic race from the Aryan or Hamitic; more narrowly still, the Arab from the Hebrew or the Celtic race from the Teutonic. Similarly there is on the one hand "the little Manx nation" and the vast nation of the United States, or the compact, but complex, Spanish nation, including the distinguishable Gallego of the northwest, the more distinct Catalan of the northeast and the northern Basque of mysterious and radically different origin.

In these and most other instances language aids in fixing the boundary; but it cannot be taken as an absolute and conclusive test of nationality or racial identity; or as measuring the territorial range of those human ties and enthusiasms, which we are bound, at our peril, to respect. The Prussian and Bavarian are quite different physically and temperamentally, yet both speak German and feel a Germanic identity, even though they may disagree provincially. Among us the negro and the white man speak the same tongue and are devoted to the same country, though certainly under no illusion as to identity of race. Again, Switzerland has three or four languages, yet no one language, but nobody doubts the unity or patriotism of Switzerland. Belgium, too, is made up of Walloons and Flemings, differing greatly from each other in race as well as language; but we do not need to be told that Belgium is a most constant and tenacious little nation of a real and noble identity.

It is apparent that classification of peoples for practical purposes must take into account most controllingly the instincts, beliefs and aspirations of men. It may matter little whether an Alsatian peasant uses French or German in his daily speech or whether his skull be square or boat-form, provided his heart turns toward France and he counts himself a Frenchman. A dweller in Posen may have had nominal Prussian nationality and an equipment of German customs fastened upon him from childhood, yet remain enthusiastically a Pole, even though Poland has long been reduced to fragments and his particular fragment lies outside even of the name. Of course bodily and mental characteristics distinguish different peoples more or less uncertainly and a common language is one of the greatest bonds to hold a nation together; but both must yield first place to the strong spontaneous selection of patriotic recognition, to what has lately become known as national self-definition or self-determination. Like many other problems which elude *a priori* solution, nationality in this sense may usually be easily brought to a practical test. In any given case, the overriding rough-shod of such patriotic self-consciousness evokes a feeling of outrage and sacrilege in the affronted people. There is no surer cause of war and widespread suffering, and the best hope

of avoiding future calamity lies in such redistribution of nations and boundaries as will end or greatly lessen domination of the unwilling and make aggression more difficult.

Even after so many later tragedies, it will be remembered that this war began with the attack of Austria on Serbia; the immediate occasion being the killing of an Austrian archduke in a Bosnian city by a young Bosnian man of Serbian blood. But this provocation had itself its provocation in the occupation, arbitrary and cruel domination and annexation by Austria of the provinces of Bosnia and Herzegovina, ancient Serbian territory inhabited by Serbs. Back of this lay the traditional Austrian policy of holding and ruling alien unwilling peoples, an oppressive system from which at different epochs both Switzerland and northern Italy had successively broken away, to their great prosperity and advancement. Back of it also lay the German conceit of racial superiority and sense of a mission to control, reorganize and exploit. This had been greatly developed and intensified since the successful and profitable defeat of France in 1870. The heart of it lay in Berlin: It claimed justification in the danger, real or supposed, from the very numerous and prolific Slavs in the East, led by Russia. It saw opportunity in the weaker southeastern national units of that race and in the ill-organized people that lay beyond. Undoubtedly there were other motives to hostility, for example, a thorough understanding of French resentment and grief over the loss of Alsace and Lorraine with a strong desire to make reprisal impossible, and impatience of the growing power of the English-speaking nations on and beyond the seas, which seemed a final bar to equal German expansion. Into all of these, racial antagonism largely entered, but perhaps nowhere else is it quite so naked and salient as in the onslaught which has desolated Serbia and Poland and sends its armies ranging and looting through a great part of what was Russian territory, while still holding Bohemians, Croatians, Bosnians and other notable families of Slavs under iron Teutonic domination.

Not that any of these peoples, nor the Teutons themselves, nor any people in all the world can be called really homogeneous; for

the human melting pot has long existed everywhere; but in each of these subjected or invaded peoples, as in many others, there exists a conviction of nationality or national identity and racial affiliation, sustained emotionally by an ideal of patriotism and answering the same purpose as a demonstrable community of descent and bond of blood.

History holds the key of these magical-seeming and sometimes apparently illogical national identities, making an Irishman an Irishman, even if he should happen to be partly a Dane, partly a Norman and very much more pre-Gaelic than Gaelic; making a devoted Frenchman out of half a dozen different kinds of men, including the Breton who still in part holds to his Celtic speech and the mysterious Basque whose speech and origin are probably not Aryan at all; and convincing the German that he is Teutonic, notwithstanding the occupancy of German territory by the Huns of Attila, the absorption of the Wends and other fragments of northern Slavs and the presence of great bodies of population distinctly marked with the Alpine type.

Nobody can hope to explain all these crystallizations of self definition nor relate the movements and changes of men which led up to them. They must reach in many cases far back into the beginnings of the white race, somewhere in the northern half of the great Eurasian continent. Mankind may have originated in the tropics, and one of its chief subdivisions, the Negro, has probably never willingly migrated away from them or warm regions of similar conditions and products. Afterward at indeterminate periods through long intervals various kinds of ancient men flowed into and over Europe, perhaps sometimes westward from Asia, sometimes northward from Africa. We dig up their gradually improving series of utensils and some of their bones and discourse about their horizons; which chiefly tend to show rather numerous accessions of population, perhaps with some partial extermination and certainly on the whole slow improvement. Perhaps none of the earlier peoples, unless the very most recent, conformed to our general ideal of any one of the main types of the race as known today. But they must have left representatives among still later comers and their

blood must continue to sway the conduct of European modern men, to what extent we cannot tell.

But at length in a relatively recent, but still very remote period we seem to find a white population of Aryan speech pouring out from some interior point, perhaps of western Asia, in great human waves, it may be with wide intervals of time between them and over-running Europe or various parts of Europe. There they presumably mingled with predecessors already on the ground, in different proportions and under various circumstances of climate and condition; so that either by these modifications or by preëxisting idiosyncrasies or both, the special kinds of white men came into existence, whom we identify as Celtic, Teutonic, Latin, Slavonic and others—named most naturally from the type of language, which each normally uses or has used.

Not that the movement was wholly confined to the white race. The Mongolian or Turanian and intermediate folk may have given the impulse. Quite certainly in historic times these darker people have followed the same path. The Hun forced his way through as far as France, but he came as a hated intruder among Aryan people already long settled on the ground, and the Turk and Tartar have been considerably later still. It is also true that Semitic peoples, operating from the eastern and southeastern shores of the Mediterranean, have made at various times serious incursions beyond its northern borders. But the story of Europe is mainly a story of successive inundations of Aryan-speaking populations and the interblending of them with predecessors, who adopted their tongue, developing in various modifications and in various quarters the racial and national characteristics of many distinctive peoples.

Physically the Europeans thus constituted may be divided by rough approximations into three zones—a northern zone of tall blond long-headed people; a southern zone of shorter darker long-headed people and an intermediate zone of sturdy broad-headed people, often mountaineers, of varying height and neither excessively blond nor extremely dark. Linguistically, the Celts were the farthest west, the Slavs the farthest east, the Greeks and Latins at the South and the Teutons in the middle and northward. In the lapse of centuries there have been some general changes and more

of a special local kind. On the whole everywhere the population has been darkening, perhaps tending to show that the duskier ingredients assimilated by the original Aryans were better acclimated or naturally adapted to prevail and become permanent. Also, the Greek and Celtic languages have shrunk to a small area for daily use, the latter being almost wholly confined to parts of Ireland, Scotland, Wales and Brittany, the mountainous western fringe of the continent. On the other hand, the composite and very copious English language has developed from a Teutonic stock nourished from many sources, as far apart as Latin, Arabic and Algonquin, and has spread immensely beyond the seas. The same is true as to territory, though not quite as to population of certain Romance languages, notably the Spanish. The Slavs and Teutons have long been interlocked, each gaining a little at the expense of the other so that, in this matter of language, it is hard to say where the preponderance lies.

The clashings and shakings down of the various kinds of European men have resulted naturally in the crystallization of sundry self-defined and clearly recognizable units, each of which for the sake of humanity, fair dealing and the general peace, ought to have jurisdiction over all the people understood to naturally belong to it and willingly seeking such allegiance. Beginning at the north, we have Scandinavia, which seems to need little change, except the return to Denmark of the Danish province of Sleswig, torn away rather long ago by Austria and Prussia and still held by the latter. Holland may well retain its present boundaries. Belgium obviously should be restored to full independence, with the utmost reparation that is feasible for the many wrongs committed against her, but with no great expansion of area; France should regain Alsace and Lorraine, the provinces conquered from her in 1870 and which still display for the most part a strong French leaning at least. Switzerland needs no change. Italy should acquire the Italia Irredenti of the Trentino and Istria. Poland should again be set up as a real and powerful nation with her former boundaries, including the Polish regions now dominated by Prussia and Austria. Bohemia should be freed from Austria as completely as Italy and should include the Slovaks and the Slavic populations of Moravia and Silesia.

Serbia should become the nucleus of a strong south Slavic state including the Montenegrins, Herzegovinians, Bosnians, Croats and Slovenes. Ireland should be made as autonomous as Canada, with only proper safeguards for the minority in religion and politics. Roumania should be restored to full independence with the addition of adjacent Roumanian-peopled territory in Transylvania and Bessarabia. Greece should have such Greek-speaking territory as lies outside her boundaries, but near. Armenia, Palestine and the new Arab kingdom set up in Mecca should be emancipated from the Turk and sustained and encouraged.

This list is not meant to be exhaustive, and such changes, however extensive, could not hope to remove all the causes of future quarrel. Most of them obviously depend on a very complete overthrow of the central powers. But that outcome may be hopefully foreseen as affording the best chance now open to humanity of preventing for the future any such wholesale slaughter of men, quite without precedent, as we have witnessed during the past three or four years. Such a rearrangement of peoples in accordance with their predilections would do away with most of the heartburnings that come from national aggression and make the path of the aggressor very difficult. Moreover, the league of civilization which would be able to establish this new status might easily retain a police power to enforce order "where the common sense of most will keep the restless few in awe." It is to be remembered that the world has never before been in such condition as to make the permanent rule of law a feasibility. Outside of the pale of Rome's civilization there lurked the formidable hordes of barbarians which were her bane, as the Mongols and Manchus poured in over the defenses of China. But communication and transportation are much more speedy now, the nations are more closely knit together; the world is more generally peopled. The league of civilization, once able to prevent serious disorder within it, could have nothing to fear from the outside, because there would be no outside nearer than the moon or the other planets. In such circumstances we might reasonably expect an indefinitely long reign of universal law.

WASHINGTON, D. C.

KINSHIP TERMS OF THE KOOTENAY INDIANS

By E. SAPIR

THE Kootenay kinship data here presented were obtained in May, 1916, from Paul David, an old Kootenay chief who was then visiting Ottawa on government business relating to his tribe. His Indian name is *Ganq'u'skle* "red horns."

The phonetic system employed is the standard system now in use among Americanists.¹ The sonant stops (*b*, *d*, *g*) are to be understood as intermediates.

The *ga-* of the following terms is the first person singular possessive pronominal prefix, "my." No distinctive vocative terms were given by Paul David. The pronominal forms in *ga-* were said to be used instead. Thus, *ga-d'to* "my father" (male speaking) was said by him to be also vocatively employed, "father!"

A few remarks of a linguistic nature may be ventured. *-dut'o* (no. 1) and *-dut'e* (no. 7), despite their outward resemblance, can hardly be etymologically related. *-dut'e* is doubtless a reduplicated stem; this is perhaps true also of *-dut'o*. Other reduplicated stems are *-bap'a* (no. 6), *-t'a:t'* (no. 9), and *-nan'a* (no. 13). *-cwin* (no. 5) is perhaps related to its reciprocal *-co* (no. 2). *-tca* (no. 10) and *-tci'ya* (no. 11) are doubtless related terms. *-ba't'* (no. 19) is evidently a derivative of *-ba* (no. 18); for *-t'* compare no. 16. Nos. 20, 21, and 26, ending in *-natlul*, are based on nos. 4, 5, and 22 respectively. *-'atcawa't's* (no. 25) is derived from *-'atcu* (no. 24). Nos. 27-31 are clearly derivational forms, but my ignorance of Kootenay morphology prevents me from understanding their analysis.

The Kootenay system of kinship terms offers a number of interesting features.

1. Chief among these is probably the extensive use of distinct

¹ *Smithsonian Miscellaneous Collections*, vol. 66, no. 10 (1916).

Term	Meaning		
	Male Speaking	Male or Female	Female Speaking
1. <i>ga-d'i't'o</i>	father	mother son	father
2. <i>ga'-co</i>			
3. <i>ga'-ma</i>			
4. <i>ga-n̄x'a'li</i>			
5. <i>ga'-cw̄m</i>			
6. <i>ga-ba'p'a</i>			
	{ grandmother (paternal or maternal) granddaughter daughter-in-law sister's son's wife	{ daughter grandfather (paternal or maternal) grandson	sister's son (see also no. 20) (see also no. 21)
7. <i>ga-d'i't'e</i>			
8. <i>ga-'a'tsum'i'l</i>			
9. <i>ga'-t'a't'</i>			
10. <i>ga'-tca</i>	younger brother	great-grandfather great-grandmother great-grandchild older brother	{ grandmother (paternal or maternal) granddaughter mother-in-law daughter-in-law
11. <i>ga-tci'ya</i>			
12. <i>ga'-tco</i>			
13. <i>ga-na'n'a</i>			
14. <i>ga'-x̄a</i>			
	brother's child	older sister younger sister father's brother maternal aunt's husband paternal aunt's husband (see also no. 15)	younger brother
15. <i>ga-a'tca</i>			
16. <i>ga-d'i'ldet</i>			
17. <i>ga'-gok'ut</i>			
	sister's child	mother's brother paternal aunt's husband (see also no. 14) father's sister maternal uncle's wife mother's sister paternal uncle's wife	brother's daughter brother's son sister's son (see also no. 4) sister's daughter (cf. no. 5)
18. <i>ga'-ba</i>			
19. <i>ga'-ba't'</i>			
20. <i>ga-n̄x'al̄na'tl̄l̄</i>			
21. <i>ga-cw̄m'a'-ll̄d</i>			
22. <i>ga-nwa'spa't'</i>	father-in-law mother-in-law sister's daughter's husband brother-in-law	son-in-law	sister-in-law brother-in-law
23. <i>ga'-cka't'</i>			
24. <i>ga-'a'tci</i>			
25. <i>ga'alca'wu'is</i>			

Term	Male Speaking	Meaning	
		Male or Female	Female Speaking
26. <i>ga-nwa'spa-tlnallil</i>		brother's father-in-law	
27. <i>ga'aqil-tsma'kin'ik</i>		child-in-law's parent	
28. <i>ga-xal.ga-x'niyat'u'm'a'l</i>		parent-in-law child-in-law (after spouse's or child's death)	
29. <i>ga-tuma'i'</i>		sibling-in-law (after spouse's or sibling's death)	
30. <i>gu-'ok'u-xwe'm'a'l</i>		cousin; any remoter relative of recognized blood	
31. <i>ga'-gin'k-na'amo.</i>		remote relative (exact degree of kinship not known)	

terms according to whether the speaker is male or female. The principle is not developed, however, with complete rigor or symmetry. The following table more clearly brings out the workings of the principle in Kootenay.

ENGLISH TERM	MALE SPEAKING	FEMALE SPEAKING
father.....	no. 1	no. 2
grandmother }	6	7
granddaughter }		
younger brother.....	10	11
brother's son.....	14 }	19
brother's daughter.....	14 }	18
sister's son.....	15 }	20 (or 4)
sister's daughter.....	15 }	21
father-in-law.....	22 }	6
mother-in-law.....	22 }	7
daughter-in-law.....	6	7
brother-in-law.....	23	25
sister-in-law.....	25	24
sister's son's wife.....	6	?
sister's daughter's husband.....	22	?

For other relationships the same Kootenay term is employed by both sexes. It is to be noted that only certain of the terms entered in the sex-table (nos. 1, 2, 7, 10, 11, 18, 19, 20, 21, 23, 24) are exclusively used by either male or female; the rest have a wider range

of significance that includes usages applying to each sex singly or both indifferently. The application of the sex principle is thus involved in a good deal of irregularity and criss-crossing.

2. The principle of reciprocity is illustrated in a number of terms:

grandfather.....	}	no. 6	}
grandchild (man speaking)....	}		}
grandmother (man speaking)...	}	6	}
grandson (woman speaking)....	}		}
grandmother (woman speaking)	}		}
granddaughter (woman speak'g)	}	7	}
great-grandparent.....	}		}
great-grandchild.....	}	8	}
father's brother.....	}		}
man's brother's child.....	}	14	}
mother's brother.....	}		}
man's sister's child.....	}	15	}
son-in-law.....	}		}
man's parent-in-law.....	}	22	}
man's daughter-in-law.....	}		}
woman's father-in-law.....	}	6	}
woman's daughter-in-law.....	}		}
woman's mother-in-law.....	}	7	}
wife's brother.....	}		}
man's sister's husband.....	}	23	}
husband's sister.....	}		}
woman's brother's wife.....	}	24	}
man's sister-in-law.....	}		}
woman's brother-in-law.....	}	25	}

Particularly remarkable is the fact that while the terms for uncle (nos. 14, 15) are reciprocally used (man's brother's *or* sister's child), this does not hold true for the terms for aunt (nos. 16, 17), each of the four reciprocal possibilities being here distinguished (nos. 18-21). The grandparent-grandchild relation may be most readily defined by saying that *-bap'a* applies to all reciprocal possibilities except when two females are concerned (no. 7). Similarly, a single term is used for all parent-in-law-child-in-law relationships (no. 22) except where a daughter-in-law is involved (nos. 6, 7).

3. The sex of the connecting relative is not considered in Kootenay except in the avuncular-nepotic relationships (nos. 14-21).

4. Perhaps the most interesting characteristic of the Kootenay

kinship system is the partial confusion of terms of consanguinity and terms of affinity:

man's granddaughter.....	} ...no. 6.....	{ man's daughter-in-law
woman's grandfather.....		
woman's granddaughter..	} ... 7.....	{ woman's daughter-in-law
woman's grandmother....		
father's brother.....14.....		mother's (<i>or</i> father's) sister's husband
mother's brother.....15.....		father's sister's husband
father's sister.....16.....		mother's brother's wife
mother's sister.....17.....		father's brother's wife.

The first two of these terms are readily understood as developed from teknonymous usage. The woman speaks of and addresses her parents-in-law in terms of her children; her father-in-law is her child's grandfather, her mother-in-law is her daughter's grandmother. The reciprocal usages (daughter-in-law) would follow on the analogy of other reciprocal terms. The other four terms may be thought to suggest the customary marriage of a sibling¹ pair of opposite sex to another such pair. To put this idea into more realistic terms, two male friends marry each other's sisters. On the other hand, the nomenclature may be merely due to a psychological cause, a feeling for symmetrical patterning.

5. Note the use of distinctive terms for relatives by affinity when the connecting link is deceased (nos. 28, 29).

6. The identity or practical identity of the terms for woman's sister's child (nos. 4, 20, 21) with those for son and daughter (nos. 4, 5) suggests the customary marriage of a widower to his deceased wife's sister. In other words, her sister's children are her own potential (step-)children. On the other hand, it is worth noting that "identical" cousins are apparently not classed as brothers and sisters (see no. 30).

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¹ "Sibling" indicates brother or sister.

INDO-GERMANIC RELATIONSHIP TERMS AS HISTORICAL EVIDENCE

By W. D. WALLIS

PERHAPS, a comparative study of social life, like charity, should begin at home, but it never does begin there. We first search far afield for our material before weariness brings us home. This, at least, has been the story of relationship terms. We first became aware of the importance of kinship terms among savages and then discovered that the problem lay nearer home, and in much the same form. For this the anthropologist may find fault with the students of European philology who might have gotten some inspiration for the task had they been familiar with the contributions in this field coming from an allied quarter. It must be confessed, however, that the students of European philology hold a similar brief against the anthropologists. In 1890, and again in 1895, Delbrück pointed out the importance of Indo-Germanic kinship terms as possible evidence of earlier social conditions, and O. Schrader later emphasized this point. But except for Rivers' article in volume VIII of Hastings' *Encyclopædia of Religion and Ethics*, the anthropologists seem either to have overlooked or to have disregarded these important contributions. Morgan's earlier and incomplete account of them seems to have stimulated no further research in that quarter, as both Schrader and Delbrück seem unaware of his contribution.

In the following account we propose to deal more especially with the Greek, Roman, and Teutonic terms of relationship. We shall first give some account of the terms that were used and then attempt to determine to what extent, if any, the kinship distinctions reflected distinctions current in the social or political life of these respective peoples.

LATIN RELATIONSHIP TERMS

As was pointed out by Morgan, the Romans had distinctive

terms for ancestors, both male and female, to the sixth degree. These were:

<i>Male</i>	<i>Female</i>
pater	mater
avus	avia
proavus	proavia
abavus	abavia
atavus	atavia
tritavius	tritavia

Similarly, in the descending line they recognized descendants to the sixth degree:

<i>Males</i>	<i>Females</i>
filius	filia
nepos	neptis
pronepos	proneptis
abnepos	abneptis
adnepos	adneptis
trinepos	trineptis

Remoter ancestors were spoken of indiscriminately as *maiores*, and remoter descendants as *posteriores*.

The importance attaching in the social and religious life to ancestors, and scarcely less, as among the Jews, to descendants as well, may account for this extensive terminology. Nor is the validity of this social-religious causation set at naught by the fact that the ancestor-worshipping Chinese, like the ancestor-respecting and descendant-hungering Jews, have specific terms for only three generations of ancestors, and as many for descendants. The motive is none the less a real one though it is not always followed to its logical conclusion.

The maternal-paternal distinction is recognized, and uncles and aunts are traced to the fourth degree:

<i>Paternal uncles</i>	<i>Maternal uncles</i>
patruus	avunculus
patruus magnus	avunculus magnus
propatrius	proavunculus
abpatrius	abvunculus

Paternal aunts

amita

amita magna

proamita

abamita

Maternal aunts

matertera

matertera magna

promatertera

abmatertera

A descriptive terminology carries the distinction still further. A nephew is referred to as either *fratris filius*, or *sororis filius*, related to me through my sister or through my brother. Cousins are *fratres patruales* if their fathers are brothers, *fratres consobrini* (or *consororini*) if their mothers are sisters. They are *fratres amittini* if the father of one is the brother of the other's mother, that is, if the parents through whom the relationship is traced are of different sex. The children of cousins german, that is, those whose fathers are brothers or whose mothers are sisters, call each other *sobrinius* or *sobrina*. To the cousins german of my father or of my mother I use the term *proprior sobrino*.

In addition to the above we find the following terms:

nurus, wife of son, of grandson, or of great grandson.

levir, husband's brother.

janitricēs, husband's brother's wife.

enater, husband of deceased sister.

socer, father-in-law.

glos, husband's sister.

fratria, brother's wife.

The writer has not been able to discover any terms for such relationships on the side of the wife. This emphasis of the relationship on the side of the husband is what we might expect in a society where the wife joins the husband's people, is under their jurisdiction and is brought into constant association with them.

In Roman law descent of property was counted solely through the paternal relatives until the pretorian law introduced succession based on blood relationship and Justinian finally removed all agnatic prerogatives. The relationship terms subsequently lost their connotation of maternal or paternal, though whether this loss of distinction was directly related to the loss of paternal privileges is a matter we have been unable to determine. The answer awaits one who is qualified to investigate this special field.

GREEK RELATIONSHIP TERMS

In Greek we find similar distinctions, the parents of the Latin terms, which have, in most instances, been derived from the Greek, often without change of meaning.

The maternal-paternal distinction is clearly observed. *θεῖος*, mother's brother; *πάτρις*, father's brother; *πατροκασιγνήτος*, the son of father's brother. Brother and sister are called *ἀδελφός*, and *ἀδελφή*, respectively, the maternal relationship being connoted in the stem *δελφός*, meaning "womb." The son of a brother or of a sister is called *ἀδελφιδέος*, and a daughter of brother or sister is called *ἀδελφιδῆ*. Thus the terms for nephew and niece preserve the common connotation of relationship through the same female ancestor. Again, in the word *πασίγνητας*, meaning a brother by the same mother (from *ἀγάστωρ*, from the (same) womb), we find the emphasis placed on relationship through the female line. This became the *agnatio* of Latin, which there meant, not maternal but paternal relationship, suggesting a change in the method of reckoning descent, though there seems to be no historical evidence on the point.

The maternal-paternal distinction is continued in the terms:

<i>μητρομήτωρ</i> ,	mother's mother.
<i>πατρωμήτωρ</i> ,	father's mother.
<i>πατρωπάτωρ</i> ,	father's father.
<i>μητροπάτωρ</i> ,	mother's father.
<i>μήτρως</i> , <i>μητράδελφος</i> , <i>μητροκασιγνήτος</i> ,	mother's brother.
<i>πάτρως</i> , <i>πατράδελφος</i> , <i>πατροκασιγνήτος</i> ,	father's brother.
<i>πατροκασιγνήτη</i> ,	father's sister.
<i>μητροκασιγνήτη</i> ,	mother's sister.

In the earlier literature the distinction is clearer than in the later literature. Thus, in the *Iliad* we find *μήτρως*, and in Pindar *μητραδελφός* used to denote mother's brother, and *πάτρως* to denote father's brother. They were replaced in the later literature by *θεῖος*, which referred to any uncle.¹ There was the same loss of the distinction formerly observed between maternal and paternal aunts; and between maternal and paternal grandparent, *πάππος*, in Plato

¹ *πατρός-θεῖος* was used for paternal uncle.

referring to a grandfather of either line. *πατρικός*, or *πάτρικος*, 'derived from one's father,' came to mean hereditary—our 'patrimony.' *οἶκος* was a term used to include the four generations composing the family group.

In addition to the above we find the following terms:

νύος, daughter-in-law; bride; wife; any female related by marriage.

ἐκνρός, step-father; father-in-law.

πενθερός, father-in-law; brother-in-law; son-in-law; any male related by marriage; plural, parents-in-law.

δαήρ, husband's brother.

γάλως, husband's sister; brother's wife.

πενθερά, mother-in-law. Derived from *πενθέω*, to lament, to bewail.

ευνάτερες, reciprocal term used by the wives of brothers. (Liddell and Scott, 8th edition, adds, "or of husband's brothers," which means presumably, a woman and the wife of her husband's brother). This term seems to be derived from *ἐνός*, ninth day.

ἀέλιοι, reciprocal term used by the husbands of sisters.

πενθεριδής, step-father's son.

ἔκνρα, mother-in-law.

γαμβρός, son-in-law; sister's husband; wife's brother; father-in-law; any male related by marriage.

ἡδεστά, son-in-law; father-in-law.

ἀνεψιός, cousin; nephew. From this (?) is formed *ἐπόδις*, 'descendants.' (Cp. Sanskrit, *napot*.)

μητρικά, *πατρώς*, or *πατρυιός*, step-father, was equivalent, in adjectival form, with 'unkindness.'

ὁμόπαις. (*ὁμός*, common, joint, *παις*, child.) Twin brother or sister.

ὁμοπάτριος, (α) born of the same father.

ὁμομήτριος, (α) born of the same mother.

πάππας = our *papa*; a term used by a child.

μάμμα = our *mamma*; a term used by a child. (This and the preceding are represented in Latin.)

θγύ-παις, a term applied to one who had given birth to a girl.

The terms *μητρννυμικός*, named after one's mother, and *κατρννυμικός*, named after one's father, were employed only in post-classical times.

The importance of the maternal-paternal distinction is reflected, in Ancient Greece, in the property rights and inheritance rights of agnates and cognates. Adoption conferred, as in Rome, all the rights of blood relationship, but males inherited to the exclusion

of females as far as inheritance could be counted. Demosthenes (46, 18) tells us that if there be no brother by the same father, nor father, nor grandfather on the father's side, to inherit the deceased's property, it goes to the nearest male relative. Whether this nearest male relative is reckoned on the paternal side to the exclusion of the maternal, we are not told. The inheritor of the property becomes, in the absence of nearer relations, the guardian of the woman and must either himself marry her or give her in marriage to some other.

According to Jebb (R. C. Jebb, *Selections from Attic Orations*, p. 381. London, 1906), the Greeks recognized four degrees of kinship, with corresponding inheritance rights. At death, property descended in the following order:

1. To brothers of the same father and to the children of such brothers. This was the first degree of kinship.
2. To sisters having the same father and to their children. The second degree of kinship.
3. To first cousins on the father's side and to their children.
4. To relatives on the mother's side by the same rules of descent that apply to the three classes given above. These constitute the fourth degree of relationship.

Do these terminologies indicate priority of descent through the mother in name or privilege?

Herodotus found among the Lycians, near kin to the Greeks, descent through the mother both of name and of property. The inheritance and the status of the children were traced through her. The Swiss classical scholar, Bachofen (*Das Mutterrecht*, Stuttgart, 1861) championed the theory that women had dominated in the earlier days of Greece and governed the state as well as the home and the farm. He based his argument on traditions that assign to woman a much more important rôle than was permitted her in historical times. One of these traditions assigns to women in primitive Athens the right to vote and to hand on their names to their children. Athene, the foundress, quarreled with Poseidon; the city was inundated, and these exclusive suffrage rights were removed. But, until the time of Cecrops, says the tradition, chil-

dren bore the name of their mothers. The accounts of Amazon warriors also impressed Bachofen, as did the important rôle of women serving as priestesses and the large influence of the female in the Pantheon.

There are evidences, though not completely convincing ones, of female descent in early Sparta. Thus, Herodotus tells us that the kings decide about the maiden who inherits her father's property (matrilineal?) "namely, who ought to have her, if her father have not betrothed her to anyone" (a record of later paternal jurisdiction?). (Herodotus, vol. vi, p. 57.)

Plutarch, however, denies that Aristotle is right in his allegation that Lycurgus

endeavored to regulate the lives of the women, and failed, being foiled by the liberties and command which they had acquired by the long absences of their husbands on military expeditions, during which they were necessarily left in sole charge at home, wherefore their husbands looked up to them more than was fitting, calling them Mistresses. (Lycurgus, 14.)

Further suggestion of maternal descent is found in Lycurgus's alleged decision to avoid all suspicion by leaving the country and traveling until his nephew should be grown up and have an heir born to succeed him. (Plutarch, *Ancient World*, 117 f.)

Mr. Rose has denied that we have any actual evidence of mother-right in ancient Greece (*Folk-Lore*, London, 1911) but more recently Mrs. C. G. Hartley's (*Age of Mother Power*, 1914) has championed the theory of Bachofen. If such a previous system of descent of name was practised we have a rationale of these relationship terms. If kinship was counted only through the mother we would have a system of terms denoting such relationship. If, later, it came to be counted through the father, new terms would be used for the new relationship and the two systems would exist side by side. Even when the importance of this distinction has passed away, the inertia of the language, despite the social changes, would keep the verbal distinction alive for a long time; finally, the verbal distinction dies down to the undifferentiated level reflected in the social system. Such a transition would explain the fact that while brother and sister by the same father might marry, this was

not permitted brother and sister related through the same mother; as also the fact that marital relations between a mother and son constituted incest, but not between a father and a daughter. (Plato; Demosthenes; Euripedes.)¹

GERMAN RELATIONSHIP TERMS

Teutonic peoples once made the distinctions with regard to paternal and maternal relatives, and the words denoting these distinctions have for the most part survived, though the distinctions themselves have disappeared. At the present time *Muhme*, *Tante* or *Base* mean, indifferently, "aunt"; at one time, however, *Muhme* meant "maternal aunt" and *Base*, "paternal aunt." So *Oheim* was "mother's brother"; *Vetter*, "father's brother," both now meaning uncle, either maternal or paternal, as does *Onkel*. *Oheim*, like *Onkel*, is probably derived from *avunculus*, while the common stem in *Vetter* and *Vater* is apparent.

If the etymology of the German kinship terms indicate a history, that history is a curious one. *Oheim* seems related to the Frisian *Ehm*, meaning mother's brother, and both of these to the Gothic *Awo*, "grandmother." *Aidem*, the old German word for "son-in-law," or "father-in-law," later restricted to "son-in-law," seems derived from *ei*, "oath." *Tochterman* also describes the "son-in-law" relationship, but this is a later form. *Enkel*, meaning "grandchild," seems related to the older form *Ahnen*, meaning "ancestors," and to *akna*, the feminine form, which seems cognate with *anus*, old woman.

In other ways, too, the etymological history smacks strongly of matrilineal descent. *Geschwister*, meaning brothers and sisters in the aggregate, is from the same stem as *Schwester*, sister. *Geschwisterkind*, it may be noted, came to mean not only nephew and

¹ In the Gortynian Code, probably of the fifth century, women share better than by the Athenian code. A daughter's portion is one half that of the son; a woman owns her property outright and it can not be taken by husband or children. At her death it is transmitted by the same kinship rules that apply to males. Even so the law showed no preference to males, the property going 1) to children, grandchildren, or great grandchildren; 2) brothers, their children or grandchildren; 3) sisters, their children or grandchildren; 4) next of kin; 5) any one of the family group, *e. g.*, the serfs on the estate.

niece but also first cousin. On this stem are constructed the in-law relationships: father-in-law is *Schwiegervater* (older term *Schwäher*); brother-in-law, *Schwiegerbruder*; sister-in-law, *Schwieger-schwester*; mother-in-law, *Schwiegermutter*; while *verschwiegert*, "be-sistered," means related to. Brother-in-law was denoted also by the term *Schwager*, which had previously referred to father-in-law and son-in-law as well. *Neffe* meant in Middle High German a sister's son, an uncle, occasionally a brother's son, or male relatives in general. Other terms represent a curious grouping, due no doubt to an extension of the original meaning of the term. Thus *Muhme*, which meant originally mother's sister, came to mean a female cousin, a sister-in-law, or any female relation. *Vetter* referred to father's brother, then to father's brother's son (in Middle High German), and later came to mean male cousin, instead of cousin by male descent, as *Muhme* came to mean female cousin, instead of cousin by female descent. *Vettergunst* came to mean nepotism, favor shown to one's relations, and *vetterlich* meant cousinly or cousinlike, a record of the later favoritism shown to this relative, though previously that favoritism had been shown to the father's sister's son. In like manner *Base* was extended to the daughter of the father's sister, to her female descendants, and came to be used for female cousins generally. The father's sister must have been troublesome and a meddler for *basenhaft* acquired the meaning of gossipy.

There seems no doubt that the original distinctions were a record of social status. The in-law relationship terms were originally used only by the wife when addressing her husband's kindred, in whose group she had gone to live.

"The Aryan terms for affinities took shape only as applied to the young wife's relation to the kindred of the man into whose home she had come." (O. Schrader, in an article on the Family (Teutonic) in Hastings' *Encyclopædia of Religion and Ethics*, vol. v, p. 752.)

It is noteworthy that while there is in early German no term for mother's brother, who was not of the kin group of her children, there is a term for father's brother, who was of the kin group—in Old High German *fatureo*, from Latin *patruus*. The wife used

also the terms *Zeihhur*, husband's brother, and *snura*, daughter-in-law, but no corresponding terms were used by the husband.

In the earliest Teutonic and Slavic dialects there is a special term for widow but none for widower. A distinct status was given the wife of the deceased, inasmuch as she might not marry until the end of a prescribed period, whereas the widower could remarry at any time. Thus the kinship distinction follows in the wake of a social distinction and performs a real service.

According to Karl von Amira (in *Grundriss der Deutschen Rechts*, vol. v), the distinction in early Germanic social life between cognates and agnates had its origin in the blood-bond, *Blutsverband*, which held together those related on the father's side, but did not include the relatives of the mother, who belonged to another blood-bond. In Scandinavia this included the descendants of the four great-grandfathers and four great-grandmothers, all rights being counted through the father's side, and not till this was exhausted, through the mother's side. A narrower group included the "most convivial six hands," *den gesippptesten Händen*, i. e., father, mother, sister, son, daughter, brother, while a broader group comprised also nephews and nieces "in the widest sense"—though von Amira does not explain what he means by "the widest sense." In this group the male descendants of *Enkeln*, father's brother, were called second sons, and were included in the first *Knie*, as were also the male ancestors of grandparents, called second fathers. The children of *Geschwister* and the *Geschwister* of parents were reckoned in the first *Knie*.

(The view, that these distinctions in kinship terminology are closely related to the social conditions and dependent upon the latter, has been advanced by O. Schrader (*Reallex. der Indogermanischen Altertumskunde*, and in *Sprachvergleichung*) and by Berthold Delbrück (*Die Indogermanischen Verwandtschaftsnamen. Ein Beitrag zur Vergleichenden Alterthunskunde. In Abhand. der Philologisch-Historischen Classe der Königlich Sächsischen Gesellschaft der Wissenschaften*, vol. II, pp. 381-606, Leipzig 1890. Also, *Das Mutterrecht bei den Indogermanen*, in *Preussische Jahrbücher* (1895), vol. 89, pp. 14-27. See also W. H. R. Rivers' article on

"Mother-Right," in Hastings' *Encyclopædia of Religion and Ethics*, vol. VIII.)

There seem, then, two ways in which these distinctions between maternal and paternal relatives can be accounted for. The first supposes a prior matrilineal or patrilineal stage where descent was counted only through one parent. When the descent came to be counted, subsequently, through both parents, the earlier method was supplemented rather than supplanted by the latter and lingered on by virtue of the inertia of language. Finally, the importance of the maternal-paternal distinction died out, and with the rationale, passed away the discrimination which originally was embodied in the terminology. Something of this progress can be traced in Greek, in Latin, and in German.

A survey of the systems of relationship used by various peoples, who are still in the matrilineal stage, shows us, however, that the distinction between maternal and paternal relatives may be made prior to the transition to the paternal. Hence, a double nomenclature cannot be accepted as marking a transition stage, though it cannot be denied that the supplementary patrilineal or matrilineal system paves the way to patrilineal or matrilineal descent and facilitates, even if it does not insure it. The fact that many peoples in the matrilineal stage have the two-fold system of descent is no blow to this argument. Greek myths are as likely to have been called forth by the kinship distinctions as to have arisen out of conditions when mother-descent prevailed.

Another rationale for the distinction will be found if we suppose the people divided into exogamous portions, so that father and father's relations belong to one portion, mother and mother's relations to another. The phratric and gentile systems of Greece and of Rome furnish such conditions; so do the kinship groups or village groups of the Teutons. (There is similar evidence from the Chinese.) The oath or in-law relationship of the early Germans is a good instance of the attitude toward an outsider unless he becomes a sworn kin-man.

Whether such fundamental units of the social organization gave rise to the kinship distinctions, or whether, on the other hand, the

kinship distinctions were primary and fundamental, and so gave rise to organizations which were, originally, only the kin, we cannot, in the absence of evidence, even surmise. It is enough to point out the correlation and the more or less parallel development of the two. Either one of these conditions could easily give rise to the other, and either one may be regarded a logical development from the other. The fact that near relatives, whether counted by paternal or by maternal descent, cannot marry, establishes exogamy, and, of necessity, exogamous groups, so that a restriction in marriage based upon blood relationship through one, or through both parents, lends countenance to a distinction between maternal and paternal relatives. Property rights emphasize the distinction and help to perpetuate it.

In conclusion it may be pointed out that the Teutonic distinctions find their analogue elsewhere in Europe. The old French ballads frequently refer to a preference for a sister's son rather than one's own son, though French does not afford the kinship distinctions that we find in German. In early English, however, we find many similarities to the Teutonic terminologies. Thus, before 1600 niece commonly meant a grand-daughter as well as the daughter of one's brother or sister, or was used to refer to any female relative. It was euphemistically applied to the illegitimate daughter of an ecclesiastic. In the sixteenth century *nepote* was used to refer to a grandson as well as to a nephew. In the seventeenth century the word nephew commonly referred to a grandson. This is probably a survival of the earlier meaning of a descendant of remote or unspecified degree of descent, and in law, a successor; suggesting that the nephew had formerly inherited the property, though neither maternal nor paternal nephew is specified. A record of this preference for the nephew is contained in the meaning attaching at the present time to the word *nepotism*, a term which was introduced to describe the Popes' and other ecclesiastics' favoritism toward nephews—as well as later toward other relatives—in giving them advancement over others. Again, though in the United States the term aunt is frequently applied endearingly to some elderly woman of no relationship, and often of inferior rank, in Eng-

land it formerly was used of an old woman who was a prostitute or a gossip, the last mentioned meaning being identical with the German *basenhaft*, derived from *Base*, father's sister.

The English word cousin, also, has had a variety of meanings. In the fourteenth century it referred to any collateral relative more distant than a brother or sister, most frequently denoting nephew or niece. Through the fifteenth, sixteenth, and seventeenth centuries it applied, in legal language, to the person of nearest relationship, including direct ancestors and descendants other than parents and children, another suggestion that the original descent of property was to the nephew or niece, since this was earlier the most prevalent meaning of the term. So the phrase "to call cousin" became equivalent to "claim relationship with," and still is current and has this meaning in England. Similarly, cousinage, now obsolete, came to mean kinship, consanguinity, or kinsfolk.¹

CAMP LEE, VA.

¹ An account of the meanings attaching previously to the English relationship terms will be found in J. A. H. Murray's *New English Dictionary on Historical Principles*. Oxford.

MEMORIAL TO JOHN WESLEY POWELL

BY FREDERICK S. DELLENBAUGH

AFTER four years in the Civil War, Major John Wesley Powell, minus his right forearm, which remained on the field of Shiloh, turned again to science and while geologizing in 1867 in Middle Park, Colorado, conceived the idea of exploring the thousand miles of profound canyons through which the Green-Colorado river tumbled down some five thousand feet in lonely fury from the peaks of the Wind River mountains of Wyoming towards the sea.

Around the hunter's camps of the Far West for years circulated wild stories of gloomy subterranean passages where the Colorado disappeared from the light of day, and tore on its tumultuous course, and no man lived who could of his own knowledge, controvert them, nor yet the companion tales of mighty falls from whose grasp there was no escape.

Major Powell formed his own opinion of these yarns and he resolved to act on his belief. From Green River station, Wyoming, therefore, on May 24, 1869, he started down the river with four small boats manned by resolute frontiersmen. After three months of desperate battling with the torrent the remnant of the party arrived with two boats at the appointed destination, the mouth of the Virgin river, August 30, 1869.

At a particularly ugly rapid below the mouth of Diamond creek, three of the men refused to proceed, despite the Major's efforts to persuade them that the end of the canyon must be near, and that they ran more chances of disaster in leaving. They climbed out on the north heading for the Mormon settlement of St. George, about 90 miles off. They were ambushed, and killed, near Mt. Dellenbaugh by the Shewits Indians.

Of the meager accumulation of scientific data gathered under the exceptionally difficult circumstances, most was lost, so that,



POWELL MONUMENT
THE TABLET

Photographed by Kolb Bros.

while Major Powell had demonstrated the correctness of his opinion that the canyons, one and all, could be navigated with small boats downward, he found himself without the scientific material which was the main object of his adventure.

Consequently, in a spirit entirely characteristic, he projected a second expedition which should be better provided, forewarned, and forearmed, and more able to carry on the proper exploration with some deliberation.

The second expedition started from Green River station, Wyoming, May 22, 1871, provision during the interval having been made for side expeditions to bring in food supplies at stated places. For two years this party made extensive observations and researches, not only along the bottom of the canyons of the main river, but up side canyons, tributary rivers, and on the heights as well for considerable distances back on each side as happened to be possible. On the north side and the west these operations reached to the High Plateaus of Utah, to the Grand Wash, the Virgin and Pine Valley mountains; and on the south to the towns of the Moquis or Hopi Indians.

This "*Exploration of the Colorado River of the West and its Tributaries*," eventually extended much further and developed into the "*Geographical and Geological Survey of the Rocky Mountain Region, J. W. Powell, in Charge*," merging finally in 1880, with other government surveys, to form the present Geological Survey, a monument to the common sense of Congress, and of which Major Powell for many years was director. Out of the Powell surveys also grew the Bureau of Ethnology, which he founded and directed to the year of his death.

In 1902 Major Powell died at the age of 68. On the second anniversary of his death, at a meeting of the International Geological Congress at the Grand Canyon, it was suggested that a monument to his memory should be erected somewhere along the Canyon rim overlooking the Granite Gorge, the scene of his greatest triumph over the river. The matter was brought before Congress and at the 60th meeting of that body an appropriation was made in the sundry civil act, March 5, 1909, of \$5,000

for the purpose of procuring and erecting on the brink of the Grand Canyon in the Grand Canyon Forest reserve in Arizona, a memorial to the late John Wesley Powell, with a suitable pedestal, if necessary, in recognition of his distinguished public services as a soldier, explorer, and administrator of government scientific work.

The design was to be subject to the approval of the Secretary of the Interior. The Secretary at that time was the Hon. Walter L. Fisher who immediately appointed, as his advisory committee, three long-time intimate friends of Major Powell: W. H. Holmes of the National Museum, C. D. Walcott of the Smithsonian, and H. C. Rizer of the Geological Survey. This committee entered wholeheartedly into the effort to secure the best design and the best results for the amount appropriated, and consultations in many directions were instituted. The smallness of the appropriation for so large a task was a handicap. Not only were preliminary expenses in the way of tentative designs and models to be considered but there were the very serious questions of transportation of men and materials to the Canyon. The site chosen was Sentinel Point about one mile west of Hotel El Tovar. Even the water for mixing the concrete would require to be hauled (as all water for all purposes is hauled for the hotel and other buildings at Grand Canyon station) from a point about seventy-five miles back from the rim. Although the great river is so near it must be remembered that it flows at the bottom of a gorge five thousand feet deep.

Another difficulty in working out a design was to provide against the vandalism of tourists and cowboys. There would be few of the latter at the Canyon but many of the former, and the practical obliteration by vandals of the Custer monument on the Little Big Horn was a clear warning.

The Art Commission, too, must pass on the design. At last, a design of a huge seat with a bronze record tablet set into its back, reared on a stepped platform, from which the chasm could be viewed, was prepared and all requirements fulfilled, only to find that its cost was beyond the funds available. Congress refused to add anything and although the Santa Fé railway offered to transport materials to El Tovar free of cost and the Southwestern Portland Cement Company of El Paso unhesitatingly contributed an entire



DEDICATION OF THE POWELL MONUMENT, MAY 20, 1918

car-load of cement, the design had to be revised and scaled down. The seat feature was omitted entirely and a truncated pyramid, of rough-dressed native stone, with a platform reached by a broad flight of steps from the side opposite the outer rim of the promontory, was adopted by the Secretary of the Interior from designs made in his office.

The modified monument was completed on Sentinel Point, December, 1916. The bronze tablet (pl. v) designed by J. R. Marshall, with an insert of a low relief portrait of Major Powell by Miss Leila Usher, was set in the face of a low altar-like wall rising from the outer edge of the pyramid in such a way that the visitor sees it as he mounts the steps and looks out into the wide chasm.

On each side of the portrait of the leader are the names of the men of his two parties who stood by him to the end of his canyon adventure, and below is the statement:

ERECTED BY THE CONGRESS OF THE UNITED STATES TO
MAJOR JOHN WESLEY POWELL FIRST EXPLORER OF THE
GRAND CANYON WHO DESCENDED THE RIVER WITH HIS
PARTY IN ROW BOATS TRAVERSING THE GORGE BENEATH
THIS POINT AUGUST 17TH 1869 AND AGAIN SEPTEMBER 1ST 1872

The dedication of the monument (pl. vi) rested with the Secretary of the Interior who made up his mind to hold the ceremony while on a trip west in the spring of 1918. On May 20, accordingly, 49 years after the event the final touch was given, to the monument marking the conclusion of a great epoch in the history of the United States, the epoch of western exploration and exploratory development which closed with the romantic achievement of Major Powell.

The hasty telegraphic invitations to the survivors of the expeditions, Messrs. Jones, Hillers, Hattan, and Dellenbaugh did not allow them time to reach the scene from their distant homes, so neither they, nor Mrs. Powell, nor her daughter, nor any of the original committee on the monument, were present.¹ Fortunately

¹ Another member of the second expedition is still living also, "bale and hearty," Captain F. M. Bishop, but as Capt. Bishop severed his connection with the party at the end of 1871 and did not go into the Grand Canyon his name does not appear on the tablet.

a surviving sister of Major Powell's, Mrs. Juliet Powell Rice, came on from California, and also Mrs. L. W. Field, who was living with her husband at Green River station when Major Powell started, and who served the party their last civilized breakfast before they turned their prows down the stream.

The ceremonies were arranged by Manager Brant of El Tovar and began at two in the afternoon of May 20, 1918, by an invocation by Bishop Atwood of Phoenix. This was followed by the placing of a wreath of wild flowers on the monument by a company of young girls dressed in white. A libation of water from the Colorado was next poured by Mrs. Field. Then Governor George W. P. Hunt, of Arizona, made an address, William Farnum the eminent actor recited, and finally Secretary Lane concluded the exercises by these remarks:

Major Powell throughout his life was the incarnation of the inquisitive and courageous spirit of the American. He wanted to know and he was willing to risk his life that he might know. This was the spirit that he showed in making the hazard of his life in exploring the Colorado River canyon. Mystery did not daunt him. It was a challenge to his intrepid spirit. From boyhood he was a soldier, not merely in the brave days of his army life, but in the equally brave days of his civil life. If, as some one has said, life is a great experience and only the adventurous succeed, Powell's life was a success. His name is forever linked with the romance of the conquest of the American continent. This monument will stand for the centuries to his honor, but there should be, and there will be, a greater monument to him, erected to him by the people of the United States. For these waters will be turned upon millions of acres of desert lands to make them fruitful. The soldiers returning from our great war across the ocean will, I trust, be put to work storing and training and leading out these waters upon the great plains below, and the homes that during the centuries to come will dot what now is waste land, will be the real monument to Major Powell.

Considering that Major Powell was one of the first to urge the reclamation of the arid lands of the United States, the Secretary's remarks were truly apropos.

But nothing, no matter how big, can express more eloquently the absolute identity of Major Powell with the spirit of the wonderful river than this simple pyramid, overlooking the most difficult portion of the torrent and the deepest and most magnificent part of the whole series of great canyons.

NEW YORK CITY.

BOOK REVIEWS

METHODS AND PRINCIPLES

Primitive Man. (Proceedings of the British Academy, vol. VII.) G. ELLIOT SMITH, F.R.S. London: Published for the British Academy by Humphrey Milford, Oxford University Press, 1917. 50 pp. 3 s. 6 d. net.

This comprehensive and suggestive paper is a timely plea for the study of man's history as a closely interrelated whole and in fact a direct claim also for the essential unity of civilization (p. 2). It is the first of the kind in recent years—at least in the English language—which has come to the reviewer's attention and while it expresses quite forcibly several ideas which he has himself hitherto lacked the courage to put forth, it also oversteps what seem to him to be the reasonable bounds.

There was a time, not long past, when anthropologists had an argument over the singular or plural origin of the human species and which ended, it seems, in a victory for the monogenists. We now appear to be entering upon a discussion of the unitary or multiple origin of cultural traits and in view of the suggested analogy perhaps we ought to yield the point at once. For Prof. Smith, if I have understood him correctly, maintains that a given culture trait or complex of traits arose at a particular time in a particular place—to be specific, in the vicinity of Egypt—whence it was transmitted by simple diffusion sometimes to the ends of the earth (see pp. 28, 29, 33 and 38). At first this seems a most illuminating flash but when the actual facts of trait distribution over the world are examined in its light many difficulties arise.

Let us agree for argument's sake that all the primary inventions originated somewhere near the meeting place of Europe, Asia and Africa. Then, in view of the fact that the earth's continental outlines have remained practically undisturbed since before man began his inventive career, we should expect the oldest of his ideas to have the widest general distribution while the latest inventions, other things remaining equal, would have traveled only a short distance from their place of origin. That is, if we may venture to represent the combined time and space relations graphically, we should have something like a pyramid or a cone rising by step-like stages, built up as it were of blocks of successively smaller and smaller dimensions. The foundation block, representing the

Paleolithic culture stage, would be the largest in both dimensions. The Neolithic would be next in size and so on up to the Iron Age, let us say. Differently stated, *i. e.*, considered merely as a spatial phenomenon, we should have a distribution arrangement very similar to that observed in the organic world where a given species normally enjoys only a relatively limited geographic range while its genus, its family, its suborder, etc., enjoy successively larger and larger areas of distribution. In fact Prof. Smith himself (p. 31) expressly recognizes the possibility of some such zonal scheme. Now as a matter of fact we find no such distribution arrangement in the sphere of culture applicable to the world as a whole and yet we do find something of the sort to be true for certain specifically defined subareas as *e. g.*, the American Southwest, where the distribution of several successive styles of pottery conforms in the most beautiful manner to just this idea. Clearly therefore some disturbing elements have entered into the larger general process of diffusion until the facts of cultural trait distribution seem a tangled skein impossible to unravel.

In the first place, as far as contrary evidence is concerned, it appears that the early phases of the Paleolithic culture as such never entered the American continent at all. Either this continent was not inhabited at the time or else our theory of distribution immediately breaks down because our first American immigrants would seem to have entered the New World somewhere on the Solutrean culture horizon, *i. e.*, at the true close of the Paleolithic Age. Our theory of unitary origin may therefore still be intact. But there is a second difficulty not so easily side-stepped. We find in both Middle and South America evidences of two or more somewhat differentiated but relatively highly specialized culture centers and in addition to these distant developments there are several subcenters in the Old World itself, as for example those of the Aegean, the Ganges and the Yang-tse-Kiang basins. That is, viewing the world as a whole, we have not one but several pyramids, pyramids which so far as we can see were not in all cases directly connected with our primary pyramid, except possibly at the extreme base level. The land route from southwestern Asia to Peru is long and difficult and our supposed migrants have left no clearly discernible tracks. There remain to us therefore only two possibilities: either there was a direct oversea communication route connecting southwestern Asia on the one hand with Middle and South America on the other (and it is a very enticing hypothesis) or else our widely separated culture centers were independent developments, the resultants of other factors than those of simple

diffusion by migrating bands. In the third place our supposed primary center—if indeed it was the primary center—long ago virtually dropped out of the race, leaving the subcenters to pursue more or less divergent courses of development; and in our talk of the “rise and fall of empires” we seem tacitly to claim that the different centers have by turn held the dominant position while at the same time the rest as a rule did not simply disappear, except in a political or military sense, but continued their separate courses, having surrendered to the conquerors or rather shared with them only those things which the latter were capable of utilizing.

Now we may admit without hesitation that all of these geographically separated centers have or had many fundamental traits in common, but it is at least equally obvious that they also exhibited traits each peculiarly their own. The ends sought by all these different cultures were more or less the same, but the means of accomplishment differed greatly. Why, it may be asked, should these differences arise if culture was the resultant merely of diffusion by contact? But the answer to that question is immaterial to our purpose at present; we are interested primarily in the trait similarities that exist in certain Old World and New World culture centers where direct contact or, in other words, the process of diffusion can, as it seems to the reviewer, safely be eliminated. What other determining factors are there besides that of diffusion?

When we begin to examine the concrete embodiments of human ideas as they come to us from different times and places we become conscious of a cleavage plane running through the whole mass. Utilitarian objects, as, *e. g.*, tools, at once strike us by their fundamental similarities of shapes; whereas, by contrast, ornamental objects strike us rather by the differences revealed. In order of time, both theoretically and as a matter of demonstrated fact, the utilitarian aspect is the older and more fundamental sphere of expression and, as it seems to the reviewer, it is in a large measure determined by considerations inhering in external nature, while the other aspect, the ornamental, is determined at least in part by considerations inhering in man himself.

If a man wants to punch a hole through a piece of skin or something he requires a sharp-pointed implement, he cannot simply employ anything that happens to be at hand. If he wishes to cut the skin in two parts he needs something with a sharp edge. If he wishes to make a substitute for skin there are only a very few fundamental ways in which textile elements can be united and made to serve the purpose. If he wishes to catch a fish by enticing him to swallow a toothsome morsel at the end of a string he will, to be uniformly successful, need a hook; and

a hook is a hook and not a penholder. If a house-builder wishes to insert a doorway in a masonry wall there are only two or three possible ways in which he can do it: he may bridge the gap in Greek fashion by a horizontal span of wood or stone, he may arch it over as did the Romans or he may draw in the aperture \wedge -wise towards the top, as was commonly done in Middle America. When the American Indian perceived the value and possibility of irrigation he was obliged to tap the neighboring stream above and not below his field, his system of watering could not vary in principle from that of ancient Mesopotamia or of the modern reclamation engineer. And if a civilized man, lost in the wilds without his pocket-knife, should attempt to improvise a substitute from flint he would, to be successful, have to repeat one or more of the primitive methods of flint working. No matter what may be his knowledge and ability, there is no royal road to success, he must return to first principles or else fail. That is to say, there is a conditioning factor in nature which tends to produce like results in different parts of the world. In the commonest of trades and even in the case of so-called unskilled labor there is a "right way" of doing everything and the untutored sooner or later stumble into it, often simply because the "right way," for one thing, is generally the way involving the least effort. But, in reality, it involves more, as any seasoned workman with a spark of feeling for his job will tell us. The "right way" of performing even the most commonplace task is the way which combines the "least effort" with the best (*i. e.*, most useful) and most pleasing (*i. e.*, artistic) result. In other words, in the making of an implement as in the fostering of a culture there is not one but two conditioning factors: there is the limitation of possibilities inherent in nature and then, besides, there is a psychic element; a successful culture is in one sense simply a happy adjustment of those two factors.

But to mention this psychic factor is one thing, for an archaeologist to define it and to describe it is quite another. The old *a priori* argument about the "psychic unity" of the human race is all very well but it gets us nowhere and yet presumably no one will deny that it is there and that it is in some measure responsible for geographically separated identities of cultural traits. The workman's pride in his task well done is not a mere something handed down to him out of his professional environment, it is something given, something which partakes of the character of a hereditary trait. But let us dismiss the subject with an illustration. There is a certain class of people—and the class is not limited by either racial or geographical boundaries—who express a not-

able fondness for bright (we sometimes call them "loud") colors in dress. To all appearances this universal trait is not a cultivated taste originating at some particular place whence it was disseminated over the earth. Rather it appears to have a deeper origin for it commonly persists even in a restraining cultural environment.

In conclusion, then, it seems permissible to state that the earliest and most rudimentary features of culture are largely utilitarian and that they show world-wide similarity not merely because men think alike but because the elementary human requirements are alike and because nature imposes certain conditions or limitations as to the manner of fulfilling these requirements. Accordingly, when we discover that primitive peoples the world over for untold ages have been using the oleocranon bone of the deer or some similar animal for the purpose of making pointed implements, it seems more reasonable to suppose that this happened because this is the particular bone which lends itself to that purpose with the least possible amount of labor rather than that the practice of employing it was disseminated from some particular spot on the globe. When, however, we come to the non-utilitarian culture features, of a somewhat later date in point of origin, the case becomes a little more doubtful. Here "psychic unity" begins to figure; but speaking for myself, I am not claiming everything for it; I am prepared to divide the credit for widely separated cultural similarities more or less evenly between *psychic unity* and *diffusion*. If, for example, Peruvian art were found to embody a whole series of highly specialized Egyptian traits, let us say, then I should be disposed to agree with Prof. Smith that there had been direct intercommunication in relatively late times. But this is not the place to enter upon a discussion of that subject. And so, without categorically denying his theory of diffusion, it seems to the reviewer that neither our facts nor our theories warrant us in wholly ignoring the old arguments about "Environment" and "Psychic Unity." And while I do not venture to demonstrate it, I believe that in the long, world-wide interplay of mind and matter identical situations are bound to have arisen at different times and places so that therefore we may take it for granted that many of our inventions have been repeated not once but several times. To say that two intellects from identical premises could not draw identical conclusions seems equivalent to saying that no one intellect could ever express a sound original judgment which in turn is equivalent to saying that civilization would never have come to be.

But why speculate further on these lines? There is another fundamental point to Prof. Smith's paper the consideration of which may lead

us nearer to the truth of the whole matter. He writes on pages 2 and 21 to the effect that some so-called "primitive" ideas are not primitive at all in the sense of being ancient but instead are often crude adaptations and borrowings from a higher neighboring culture, often of a relatively late date. This of course is a fact well recognized among critical students and it is one which works havoc with our chronological schemes and in so doing shows us the inevitable danger of intensive and at the same time geographically circumscribed research. Frankly, to one contemplating the archaeology of western Europe from the opposite side of the Atlantic that science has for some four or five years presented several very astonishing inconsistencies. But let Prof. Smith speak.

After warning us of the "confusing chronological implications" in our present use of the terms Paleolithic and Neolithic he urges what he calls "the still more fundamental objection" to these and other definitions, viz., "that the great cultural break in western Europe itself (and even in its flint work) did not fall between the so-called Paleolithic and Neolithic Ages, but between the Lower and Upper Paleolithic periods (pp. 18-29). There is," he continues, "a much closer kinship between the flint-work of the so-called Upper Paleolithic and Neolithic Ages than there is between the former and that of the Lower Paleolithic period" (p. 18). This is a sweeping assertion but one which, in view of the substitution of racial types supposed to have occurred at this time, might well have been expected to be true. Nevertheless, if we are to accept the chronology as determined for France and northern Spain, the reviewer finds it impossible to agree unequivocally. Certainly, the Aurignacian industry is characterized by the seemingly sudden appearance of a whole series of new types in the way of both bone and flint implements; but traces of bone implements do occur in the Mousterian deposits (the reviewer has removed them with his own hands at Castillo) and the Aurignacian flint technique, at least in certain of its phases, is identical with that of the Mousterian epoch. In fact, if our Paleolithic data for western Europe have been correctly and completely presented, then, from the point of view of the flint-working technique, the transition from Mousterian to Aurignacian offers fewer difficulties than that of any other shift, except perhaps the one from Chellean to Acheulean. Thus really important modifications are involved in the transitions from Acheulean to Mousterian, from Aurignacian to Solutrean, and from Solutrean to Magdalenian—all quite as radical in their way as the transition from Magdalenian or Azilian to Neolithic.

Stating the condition in another way, the whole Paleolithic group

series (excepting the Acheulean) have until very recently appeared like so many distinct, successive phenomena not derivable the one from the other. It has looked as if possibly some of the transitional facies did not exist in western Europe; that in other words, these successive inventions were perfected elsewhere—in Asia or Africa, perhaps—and that they reached the Pyrenean foothill country merely as so many successive cultural waves. Thus conceived, the separate group series, in conformity to what seems to be Prof. Smith's general theory, might have originated in one and the same locality; or, what is just as probable, they might have originated each in its own separate locality; or finally, as seems even more likely, some of them might have been re-originated at several different times and places. In short the general outlook with reference to our Paleolithic culture problem has been not unlike that which the outsider still seems to see in physical anthropology and in fact throughout the whole range of evolutionary biology. That is, we have had a geologically or stratigraphically determined time series but no one has been able until recently to demonstrate anything like genetic relationships. Thus to cite an example from physical anthropology our author himself (pp. 16 and 23) considers it doubtful whether *Homo neanderthalensis* was in the direct ancestral line of modern man and with reference to the contemporary implement series he says, on the latter page, that there is nothing to suggest the evolution of one type from another as having taken place in western Europe. The whole is to him a discrete series of phenomena. To all this, however, it was always possible to reply either that the representative transitional stations had not been located or else that the data at hand had not been critically handled. But at the present moment that subterfuge is hardly necessary. The French archaeologists, particularly Breuil, Capitan and Obermaier, have all in recent years made some headway towards bridging the gaps by showing, for example, that certain Solutrean features were clearly foreshadowed in the Aurignacian industry and that certain Acheulean features held over into the Mousterian, etc. It is still possible perhaps to agree with Prof. Smith that our Paleolithic industrial series were most of them invented beyond the confines of western Europe but the transition from one to another even here is not so abrupt as we formerly supposed.

But our disagreement with Prof. Smith is only partial. He administers a well-directed blow to those mostly European archaeologists whose evolutionary ideas have led them to take for granted that the cultural history of France is the natural and necessary cultural history of the world at large. That this assumption is unfounded we have already

suggested in our reference to America but as the author himself points out (p. 19), it can be demonstrated without going beyond the confines of Europe. Thus, as he puts it, "the Mediterranean lands as a whole passed directly from the Aurignacian stage to the Neolithic," the Solutrean and Magdalenian industries never having made their way for instance into Italy and southern Spain. In the same way it now seems timely to add, there are probably other sections of the Old World in which the Solutrean stage passed directly over into the Neolithic, without the interposition of the Magdalenian and Azilian phases. And this brings us to the crux.

Prof. Smith has not told the whole story and besides, his argument has a recoil, as it were. What he said about the irregularity in geographical distribution of our western European culture phases and consequently about the "confusing chronological implications" is doubtless true. But why accept the chronological series localized in France in preference to those of Italy and those of Austria? Stratigraphy is not the only criterion of chronology; the history of culture involves technological considerations which cannot properly be ignored and which in this case appear to show the way out of the difficulty. Thus, however discrete and unrelated may seem the industrial phenomena making up the French Paleolithic series as determined by stratigraphic methods, from the point of view of technique alone there is, I am convinced, something both "natural" and "necessary" in the scheme, at least up to a certain point. That is to say, the methods followed in the production of the successive series of implements do depend the one upon the other and therefore do appear in some measure to be achieved by those "blind forces of an arbitrary and inevitable process of evolution" which Prof. Smith so repeatedly and so vehemently seeks to expose and to discredit (see, *e. g.*, pp. 19, 28, 34, 47, and 49). For just as in the study of human embryology we imagine we see passing in review the main transformation making up the history of the phylum just so in the production of a Solutrean blade the artisan is obliged to repeat the main technical steps of the preceding Paleolithic Age. During the rough reduction of his flint nodule, by means of flaking, he must employ what is essentially the Chellean method and during the finishing process, by means of chipping, must employ what is nothing more nor less than a Mousterian trick brought to perfection. In other words, if we view the Paleolithic industries in the large we see only two great technical steps—the Chellean and the Mousterian; the Acheulean is merely an advance or an improvement on the former as the Aurignacian and Solutrean are successive improvements

on the latter. For this reason—without for a moment calling in question the validity of Paleolithic stratigraphy as determined for western Europe—it seems to the reviewer self-evident that the Solutrean (not the Magdalenian or the Azilian) flint industry is the true and only possible transitional phase connecting the Paleolithic and Neolithic ages. In fact the Solutrean technique as such remains a phase of Neolithic culture to the present day. Stated a little differently, the Solutrean flint industry is the technological outcome and consummation of Paleolithic workmanship: beyond it there was nothing left to do but to fashion implements by pecking and grinding. The latter process of reduction was known already, having been used on bone implements all through Aurignacian times. Therefore, seeing how difficult it has been even stratigraphically to bridge the gap between the Magdalenian and Neolithic cultures by way of the Azilian-Tardenoisian phases, why not recognize at once that technologically it can not be done; that the Magdalenian and Azilian-Tardenoisian stages, as they appear in France, are merely survivals or successive continuations of the Aurignacian which later was temporarily obscured by the incursion of a Solutrean culture wave from some outside point of origin. Or we may explain the situation in another way. There is some reason for believing that the Solutrean technique was actually invented or perfected in the Pyrenean region and that although it did not at once gain general acceptance in western Europe it spread thence eastward over Asia and ultimately into both Africa and America. Meanwhile those who had failed to take up with the Solutrean technique went off on an impossible line of specialization and when they had achieved almost complete extinction with their Azilian-Tardenoisian efforts they were at last engulfed in a return wave of the full Neolithic. But, wherever the Solutrean center of origin, whether in France or in the region of the Black Sea, as suggested by Prof. Smith (p. 19), its center of culmination lies probably somewhere to the east, perhaps in Asia. Some day we shall discover it and when we do we shall doubtless at the same time find the commencement of the so-called Neolithic technique. For an archeologist to predict this seems no more hazardous than for the astronomers to have predicted the position of a missing planet.

With this we shall have to close. We have considered only the two main points to Prof. Smith's paper and while the discussion has been drawn out at some length, it is not, I trust, out of proportion with the importance of the subject. The reviewer has attained neither the years nor the familiarity with the facts of the subject to permit his claiming a

matured judgment; but he is prepared to go a long ways with the writer though, most emphatically, not all the way. But whatever the faults of the paper or of the review, it seems clear that we are much in need of this type of study or otherwise we shall soon be swamped in a multiplicity of details.

N. C. NELSON

NORTH AMERICA

Teton Sioux Music. FRANCES DENSMORE. (Bureau of American Ethnology, Bulletin 61, pages 1-561, plates 1-82.) Washington, 1918.

In this volume Miss Densmore presents and analyzes 240 songs, and describes the ceremonies and occasions on which they are sung. Combined with her Chippewa music in Bulletins 45 and 53, this makes a total of 600 songs which she has rendered available from the two tribes. The volume of this material renders her work a most important contribution in a division of ethnology which is still in its infancy.

Fault will probably be found in some quarters with Miss Densmore's transcriptions on the ground that she has used ordinary musical notation with only an occasional indication of pitch deviation. It is to be hoped that she will not allow such criticism to disturb her. It is no doubt important that some study of primitive music be made with a finer determination of pitch values than the current musical notation allows. But until we know more of the deviations of pitch in our own vocal music, it would be pedantic to insist that a superior standard of discrimination be applied in primitive songs.

On the other hand, the author's treatment of tonality seems open to more valid objection. In assuming the tonality of each song she obviously predetermines its scale and therefore the scales of Sioux music in general. By her non-observance of this fact, Miss Densmore seems to have vitiated entirely the value of her tables 1, 2, 3, 6, 7, 8, and 14 both in her analyses of the Sioux songs (page 12) and the comparison with Chippewa (page 26). It is true that the question of tonality and scale in primitive music is difficult, and that just because the feeling for tonality is obviously less rigid than among ourselves, and the scale somewhat looser, any method of attack is open to a certain arbitrariness. At the same time there is a definite problem, and to take our own system as the point of departure precludes any possibility of determining the native system, however vague this may be. The author's tabulations would certainly have shown more if she had attempted at the outset to ascertain the tone which in native feeling seemed in each song to correspond most closely to our tonic. This would probably be a tone brought into

prominence by repetition or accentuation, possibly by the structure of the melody; and would, with high probability, be either the first or the last note of the song. In many cases there might be doubt as between these alternatives; but the tabulation of the entire analyzed material, first on the basis of one assumption—say that such tonality as there was resided in the last note—and then of another,—say the first note,—would have provided two or more sets of interpretations of the material. Between these there might then have been a distinct choice on the ground that one method of interpretation yielded a greater consistency of system than the other. Even this plan could not be expected to carry us very far; yet it would at least have been an endeavor to obtain objective results. Miss Densmore's assumption of tonality is unscientific because it is subjective—subjective not in the personal sense, it is true, but with reference to our music.

Her tabulation of the tonal compass of the songs is free from this fault, and of interest. The same may be said of her studies of intervals and progressions. Downward progressions are nearly twice as numerous as the upward. This is partly due to the descending tendency of the songs as wholes, and partly to the fact that the intervals in upward progressions tend to be larger. The average interval is very nearly three semitones.

Miss Densmore's comparison between the older and more recent songs (pages 22 to 25, with indications throughout in the tables) is a most laudable innovation. The newer songs (presumably composed in the last half century) show some tendency to smaller compass of tone, to beginning with an upward progression and with an accented part of the measure, to avoiding change of time, and to increasing the number of distinct "rhythmic units" within a single song. Other features, as the average interval, have hardly altered. The author possibly makes somewhat more of the changes than her material warrants, and it is of course difficult to estimate the age of many songs; but on the whole there can be no doubt that she has developed in this matter an interesting and promising line of attack.

Her comparison of Chippewa and Sioux music contains some interesting results (tables 5A, 9A, 10A, 11A, 12A, 13A, 14A, and 17A) whose detailed discussion lack of space forbids. On the whole, Chippewa and Sioux music are probably quite similar. It is to be hoped that Miss Densmore will not only continue her researches along this line, but give them greater geographic range. As it is now, there is nothing in the author's work to indicate whether Sioux or Chippewa music may not

be substantially identical as compared with the total range of American systems, or on the other hand may represent extremes. Even the comparison with the smaller collections made by other authors from other tribes would have helped greatly—Miss Fletcher's Omaha material, for instance, or some of the fairly extensive series from the southwest or northwest. With Miss Densmore's experience she should have little difficulty in using the work of other observers, and allowing at least to some degree for the differences of perception or method. Or, researches of her own, based on a hundred songs from each of six tribes—say a pair from each of three widely separated regions—would have given an immensely wider outlook on native American music in general than the present six hundred songs from two contiguous tribes. And this enhanced perspective would have more than made up for any minor inaccuracies resulting from less thorough acquaintance with the several groups.

The graphic plots (pages 51 to 54) are very interesting, and will no doubt yield important results when more systematically applied. The indication in the transcriptions of the "rhythmic units," and their subsequent tabulation (page 525), is also to be commended as contributing to the clearness of structural analysis.

The major part of the volume consists of transcriptions, individual analyses, and ethnological material. This rather diverse mass of material is presented in a dovetailed manner that is unlikely to satisfy either the musical student or the ethnologist. Musically there is nothing gained, and normally something lost, by having two songs and their analyses separated by a ceremonial description or biography. Reciprocally, the student of Sioux religion will feel the same way about the interspersed songs. The author appears to have had a feeling that a song could be best studied in relation to its place in the culture. Ultimately, this feeling is correct. But in its first aspect a song presents a musical problem and must be brought into relation with other musical material. It is probably only after the music and the religion of the Sioux have been separately worked out with some care that endeavors to determine the relation between the two can be seriously fruitful.

For instance, on page 53 Miss Densmore presents five types of melodic outlines:

The character of type C is a repetition of the lowest tone, usually the keynote, the melody descending to the keynote, returning to a higher tone and again descending to the lowest tone, with a repetition of that tone. . . . It will be recalled that the element of affirmation was very strong in the treatment of the

sick. . . . Reference to the analyses of songs used in treating the sick will show a large proportion of these songs ending on the keynote. . . . Type D . . . first noted in the songs of Dream societies, . . . is characterized by a short ascent and descent frequently repeated in the melody. . . . Practically all are songs concerning men or animals in motion.

The implication of these passages would seem to be that a melody which substantially repeats its course suggests affirmation and hence confidence to the Sioux, and that one whose course wavers, suggests the motions of dream men or animals. If this is the author's interpretation, it should be received with some misgivings. It is very doubtful whether much rationalization can be expected of any normal music. Even if such an element exists, it would probably be secondary to a more objective association between a musical type and a cultural type. The Sioux, like other Indians, no doubt possess a distinctive class of songs used in shamanism, just as they have distinct types of music associated with gambling, love-making, and various kinds of ritual. The first requisite in the face of this situation however is a wholly objective determination of the type of the shamanistic songs as compared with the other types of songs. How far there may also be a connection in subjective feeling or reasoning between shamanistic music and shamanistic practice, is a separate problem, a subsequent one, and in the nature of things, an extremely delicate one. Our own church music expresses a variety of emotions in a similarity of forms. Everyone recognizes a hymn tune as a hymn tune, although one hymn professes to express joy, another resignation, and a third adoration. An endeavor to correlate among the body of our hymns particular musical forms with these several emotions would seem justified only after the normal form of our hymns as music had been established with reference to our music in general. In other words, a piece of music associated with a certain cultural activity is first of all music, secondly a piece of culture, and only lastly and indirectly an expression of personal emotion.

Miss Densmore should not feel discouraged at these strictures. She has done so much that it is impossible to resist the temptation of pointing out how by a slight change of method, and by a broadening of scope not necessarily involving a greater employment of time, she could have done even more. There are so few workers in this extremely interesting field that coördination of effort is imperative. No investigator operating in spiritual singlehandedness can attain to full fruition. Very little technique of investigation and interpretation has been established. Each student is experimenting in method—now successfully, and again mis-

takenly. Each must profit by the errors and attainments of the others. In that case it will not be long before a body of information is assembled and a mass of tried methods is available which will allow of much more rapid progress on the part of each individual collaborator.

A. L. KROEBER

The Ethnogeography of the Tewa Indians (Twenty-ninth Annual Report of the Bureau of American Ethnology). JOHN PEABODY HARRINGTON. Pages 37-636, 16 pls., 30 maps. Washington, 1916.

This scholarly work, whose size and detail approximate the monumental, deserves notice first of all as one of the few exact ethnogeographic studies published in the American field in recent years. It represents a side of research which, with the allied topics of politics and economics, has been unduly neglected. Subjects such as these scarcely lend themselves to purely schematic distinctions or to theorizing. Least of all can ethnogeographic material be made to subserve a theory of evolutionary development. It also requires an intimate knowledge rather painstakingly acquired. Much more attention has therefore been devoted by anthropologists to fields in which slender materials offered the allure of readier generalizations. But, as it is accepted that no ethnological principles can pretend to much validity that do not rest on an understanding of the involved civilizations as wholes, so no civilization can be wholly known without the geographic basis which is its soil in the metaphorical as well as physical sense. This is perhaps doubly true for the ethnologist who is not a formal environmentalist.

Our gratitude is therefore due Mr. Harrington: first for undertaking the task, and second, for carrying it through with unusual conscience, exhaustiveness, and adequacy. The literature, even if bad or indifferent, is always connected with the new data presented. The form and meaning of native names are rendered most carefully. A mass of correlated matter is brought in, although not strictly geographic. Even the mapping has been done afresh wherever existing bases were wrong or insufficient.

Only one stricture can be placed; and this on the ground of omission. Except for a few pages introducing the long section on place-names, there is no summing up, no inferences, no generalized connecting of Tewa geography with Tewa culture. The work, for all its value, remains a huge dictionary arranged geographically instead of alphabetically. The plan of presentation is carefully worked out: when the reader has appreciated this, and the quantity and quality of the contained data, and is

ready to proceed to a synthesis which his mind can carry away, the book stops. Mr. Harrington has previously shown some inclination to assemble building stones and then refrain from building; but never so pointedly. This restraint is clearly not due to lack of intellectual ability, since materials of such high grade cannot be gathered and arranged other than by a mind of keenness, insight, order, and judgment. The cause is perhaps rather a temperamental inhibition, a Bastian-like sense that the need of the hour is so pressing that the rearing of the edifice must be deferred; an overstimulated conscience, in short. But it should hardly be necessary to recall that no one but the author can ever extract the full value of the author's own observations. Even he cannot crowd into his printed lines all that his head holds on his subject. More important yet, an ethnology wholly devoid of interpretations and confined to the piling up of raw materials, would inevitably lose in very short order all claim to the support of other scientists and the public—support moral and economic. Mr. Harrington clearly possesses the capacity to interpret. If he defers doing so until his field utility is over, there is not one chance in a hundred that he will ever contribute more than hewing of wood and drawing of water to his science.

What every colleague therefore wishes is not less of the same, but a continuance with more generalization added. The Bureau of Ethnology is to be congratulated on this achievement of one of its staff, and anthropology on possessing an institution able and willing to handle a task as large and arduous as the work represented by this volume.

A. L. KROEBER

Myths and Legends of the Sioux. Mrs. MARIE L. McLAUGHLIN. With illustrations from original drawings. Bismarck, N. D.: Bismarck Tribune Co., 1916. 200 pp.

The author of this little volume is a quarter-blood Dakota, her grandmother having been a full-blood Mdewakanton Santee. As the wife of Major James McLaughlin, she had an opportunity of continuing relations with the Dakota of Devils Lake and Standing Rock, where her husband gained eminence as an Indian agent. Unfortunately she does not specify from which bands the several tales were derived but the dedication makes it probable that the bulk of the lore here presented is of eastern Dakota origin, a conclusion in some measure corroborated by intrinsic evidence. For some obscure reason Dakota mythology has until a short time ago received rather scurvy treatment at the hands of field investigators. The handful of stories published by Riggs in the

Dakota Grammar and Wissler's collection in the *Journal of American Folk-Lore* (vol. XX, pp. 121-131, 195-206) represented approximately all that was readily accessible until the publication of the volume before us and the still more recent narratives appended to Dr. J. R. Walker's paper on *The Sun Dance and Other Ceremonies of the Oglala Division of the Teton Dakota*. Accordingly, pending a full collection on scientific lines, no collection, however unpretentious, can be ignored by the specialist. Several of Mrs. McLaughlin's tales have Unktomi for a principal actor and he is several times introduced in a minor capacity. The identification of Unktomi (Iktomi) with the spider seems general among the Dakota proper, though it is lacking even among the Assiniboine. Riggs (op. cit., p. 138) somewhat over metaphysically represents this character as the incarnation of evil, but taking all the available Dakota data into consideration there can be but little doubt that Unktomi was conceived essentially as a selfish or malevolent being. The present collection confirms this view. He appears as a glutton feeding his starving family on the Pounded-man (p. 142); again he plays the part of the false suitor in clothes stolen from the hero, whom he impersonates to gain the hand of the chief's daughter (p. 162); and we find him wantonly destroying the babies of two widows and later the mothers themselves (p. 198). The only definitely benevolent action ascribed to him is the manufacture of flint arrowheads (p. 77). This almost total want of heroic traits is certainly remarkable from a comparative viewpoint. Thus Iktomi's almost namesake among the Assiniboine, while freely indulging in discreditable activities, is also associated with the creation of the earth and the regulation of the seasons. (Incidentally, it may be noted that the earth-diver episode does not seem to have been recorded from the Dakota in any connection.) Among the Crow, too, Old-Man-Coyote, while emphatically a trickster, is very clearly also the earth-moulder and the founder of native institutions.

Fragments of a Rabbit cycle suggest a connection between Omaha and Dakota lore. The Bloodclot tale (p. 80) is very similar to the Omaha version (J. O. Dorsey, *The Cegiha Language*, p. 49.) In the False Suitor story (p. 162), Mrs. McLaughlin's human hero is named White-Plume, but otherwise her variant approaches Dorsey's myth of Rabbit and Ictinike (p. 55) in all details. Two other tales in this volume are practically identical with Omaha stories—that of Raccoon and Crawfish (p. 37; Dorsey, p. 312) and the myth of Unktomi beguiling two women to search for plums (p. 198, Dorsey, p. 562). The last mentioned episode is also narrated by the Assiniboine, Gros Ventre, Blackfoot and Arapaho (see Lowie, *The Assiniboine*, p. 124).

There are naturally a number of motives of fairly wide Plains distribution. Thus, we encounter our old friends, the deserted children (p. 56), the buffalo-wife and the elk-wife (p. 170), and the transformation of a man into a water-monster from eating an uncanny animal (p. 26). The episode of the offended turtle drowning warriors who try to ride it (p. 24) has been recorded by Dr. Dorsey among the Pawnee (G. A. Dorsey, *The Pawnee*, p. 426), by Professor Kroeber among the Cheyenne and by the reviewer among the Crow. The Stone-boy myth (p. 179), of which Wissler and Walker have published Oglala versions, has also been noted among the Crow by the present writer, though in a somewhat aberrant form. Arapaho and Gros Ventre variants have likewise been published.

The Artichoke and the Muskrat (p. 29) illustrates a very interesting type of tradition—that based on a dialogue between self-laudatory and mutually vituperative speakers. This genre does not seem to have been greatly cultivated anywhere in the Plains, yet a number of tribes have at least one representative: the Cheyenne and the Omaha have the Sun and the Moon, the Assiniboine the Red Hawk and the Black Hawk, the Crow the Wolf and the Dog as the hostile interlocutors.

It is clear that Mrs. McLaughlin's book merits the attention of the Americanist, but even when we combine her data with all the other available Dakota folklore the hopeless inadequacy of the material from this tribe remains tantalizing. We know from Riggs that the Dakota had a version of the Star-boy, the Bead-spitter, the hoodwinked birds, Potiphar's wife and the splinter-foot girl myths; and Wissler's notes prove the occurrence of Turtle's war party and the transformation of Coyote into a buffalo (in a version that recalls the Omaha equivalent, Dorsey, p. 105); but as yet it would be premature to make any general statement on the special affiliations of Dakota mythology and any negative conclusions would be distinctly hazardous.

ROBERT H. LOWIE

SOME NEW PUBLICATIONS

Bogoras, Waldemar. *Tales of Yukaghir, Lamut, and Russianized Natives of Eastern Siberia.* (Anthropological Papers of the American Museum of Natural History, vol. xx, pt. 1, pp. 1-148.) New York, 1918.

Brigham, Wm. I. *Additional Notes on Hawaiian Feather Work; Second Supplement.* (Memoirs of the Bernice Pauahi Bishop Museum of Polynesian Ethnology and Natural History, vol. vii, no. 1.) Honolulu, 1918. Pp. 69, 54 figs., 4 pls.

Densmore, Frances. Teton Sioux Music. (Bureau of American Ethnology, Bulletin 61.) Washington, 1918. Pp. 561, xxviii; 82 pls., 43 figs., 240 songs.

Goddard, Pliny Earle. Myths and Tales from the San Carlos Apache. (Anthropological Papers of the American Museum of Natural History, vol. XXIV, pt. 1, pp. 1-86.) New York, 1918.

Gray, Louis Herbert, *ed.* The Mythology of all Races, vol. III: Celtic, by John Arnott Macculloch; Slavic, by Jan Máchal; Baltic, by the Editor. Pp. x, 398; 37 pls.

Hrd'ička, Aleš. Recent Discoveries Attributed to Early Man in America. (Bureau of American Ethnology, Bulletin 66.) Pp. 65, 14 pls., 8 figs.

Moore, Clarence B. The Northwestern Florida Coast Revisited. (Reprinted from the Journal of the Academy of Natural Sciences of Philadelphia, vol. xvi.) Philadelphia, 1918. Pp. 513-579, 42 figs., pls. XIII-XVI.

Nelson, N. C. Chronology in Florida. (Anthropological Papers of the American Museum of Natural History, vol. xxii, pt. II, pp. 75-103.) New York, 1918.

Perry, W. J. The Megalithic Culture of Indonesia. Manchester: The University Press (Longmans, Green and Co., London, New York, Bombay), 1918. Pp. xii, 198; 9 figs., 4 pls., 4 maps.

Sarasin, Fritz. Neu-Caledonien und die Loyalty-Inseln. Reise-Erinnerungen eines Naturforschers. Basel: Georg und Co., 1917. Pp. x, 284, 184 figs., 8 pls., 1 map.

Spier, Leslie. Notes on some Little Colorado Ruins (Anthropological Papers of the American Museum of Natural History, vol. xviii, pt. IV, pp. 338-362.) New York, 1918.

Sullivan, Louis R. The Bearing of Physical Anthropology on the Problems of Orthodontia (Dental Cosmos, April 1918), pp. 11.

Id., Racial Types in the Philippine Islands (Anthropological Papers of the American Museum of Natural History, vol. XIII), pt. I, pp. 1-61, 2 maps, 6 text figures. New York, 1918.

Teggart, Frederick J. The Processes of History. New Haven, Yale University Press, 1918. Pp. ix, 162.

Tello, Julio C. El uso de las cabezas humanas artificialmente momificadas y su representacion en el antiguo arte peruano. Villarin: Lima, Peru, 1918. Pp. 60, 11 pls., 37 figs.

Thomas, W. I., and Znaniecki, Florian. The Polish Peasant in Europe and America; Monograph of an Immigrant Group. The University of Chicago Press, 1918. 2 vols. Pp. xi, 526-589.

West, Carl J. Introduction to Mathematical Statistics. Columbus: R. G. Adams and Co., 1918. Pp. 150.

Wissler, Clark. The Sun Dance of the Blackfoot Indians (Anthropological Papers of the American Museum of Natural History, vol. xvi, pt. iii, pp. 223-270). New York, 1918.

DISCUSSION AND CORRESPONDENCE

POLYNESIAN TOMBS

DR. RIVERS could not write about "Sun Cults and Megaliths in Oceania"¹ without stimulating new ideas. A suggestive paper however invites criticism as all the suggestions it contains cannot be of equal value; the inquiries to which they give rise confirm some and reject others.

The part of his article I would modify before accepting is that where he draws a comparison between the *marae* of Polynesia and the *nananga* of Fiji.

The *marae* were sacred enclosures containing pyramidal structures; they were places of sacrifice and worship. Among the religious emblems they contained there have been noted images of the sun. The *nananga* were also sacred enclosures with a pyramidal structure. They lay outside the village, and were the scene of the cult called *Mbaki*, that is Year or Crop. This cult was so secret that little is known about it, and that little is scarcely certain. The secret ceremonies of the *nananga* were followed by a public performance on the village green. This cult was not common to the whole of Fiji but confined mainly to the valley of the Singatoka river and to the south and west coast of Viti Levu; the tribes that practised it were all members of that group which I have called "Low Fijians."

Dr. Rivers argues thus:

The *nananga* resemble the *marae*;

The *marae* were devoted to the worship of the sun;

Therefore the *nananga* were also the scenes of sun cult.

In ethnology as in any other science we must argue from the known to the unknown. The known is modern European society, the society in which we live and which we know as a whole and in its past; the unknown is that extinct Polynesian society which we are trying to reconstruct from the meager fragments that remain. Before we can apply any argument to those fragments we must test it on European culture; if it leads to conclusions at variance with known facts, it is a faulty argument and must be cast on the rubbish heap. Before we can accept Dr. Rivers's reasoning let us see what it makes of our own temples.

¹ *American Anthropologist*, (n. s.), vol. 17 (1915), p. 443.

Notre-Dame in Paris is a Gothic structure; Westminster Abbey is in the same style. Notre-Dame is dedicated to Roman Catholic worship; therefore Westminster Abbey is also Roman Catholic. This we know to be contrary to facts.

Certainly Westminster Abbey was once Roman Catholic; but it is no longer so, and that is quite enough to deprive Dr. Rivers's argument of all force. Besides there are plenty of Gothic churches and chapels which were originally built by Protestant sects, Methodists, Congregationalists, and even Unitarians. The Gothic is spurious, if you like, and easy to distinguish from the real mediaeval Gothic; still it is Gothic, whereas the cults which it harbors are far removed from Roman Catholicism and in some cases have almost ceased to be Christian. The cults all belong to the same family and have the same origin, even as the architectural styles, but they have diverged so much more widely that one is no very good guide to the other.

What is true in Europe is true at the Antipodes. The *marae* and the *nananga* may be ever so much alike, it does not follow that they are associated with the same doctrines. The most we can say is that probably those doctrines, like the architecture, have everywhere the same origin, but while the style of building may have changed but little (as far as our evidence goes it may have changed considerably) the doctrines may have traveled so far apart that they scarcely retain a point in common. If Unitarians can build Gothic chapels in which the Virgin and the Saints and even the Trinity have no part, the Fijians may well have set up pyramids to other gods than the sun, or to no god at all. There is no such proportion in Ethnology as:

Style A : style B = religion M : religion N.

So much for critique. Let us see what the facts have to say.

Tregear in his *Comparative Dictionary of Maori* gives the following meanings of the word:

Maori: An enclosed place in front of a house.

Samoan: An open space in a village.

Tahitian: The sacred place formerly used for worship, where stones were piled up, altars erected, sacrifices offered, prayer made, and *sometimes the dead deposited*.

Hawaiian: A calm place in the sea.

Tongan: A green.

Mangaian: The sacred enclosure.

Mangarevan: Sacrifice, first fruits.

Paumotuan: A temple.

I would like to add that in Wallis island and in Rotuma the meaning is the same as in Samoa and Tonga. It is difficult at first sight to find consistency in all these various definitions or to guess at the original fundamental meaning of the word. The key is supplied by Tonga, where I found a use of the word not recorded in any works I have seen. I was there told that the king's tomb was called a *langi* or heaven; a chief or nobleman's was known as a *mala'e*; for the common people the term was *faitoka*.

I will here refer the reader to my paper on *Chieftainship and the Sister's Son in the Pacific*,¹ in which it was shown that Polynesian and Fijian chiefs and kings are divine. Now if,

Chiefs = gods,

then

Chiefs' tombs = temples.

All the various definitions given by Tregear derive naturally from this equation. The Tahitians often buried their dead in the *marae* because a chief's grave, a temple, and a sacred enclosure, were all one. The village green is called *malae* or *marae* because it was the open space before a chief's house or a temple. The Mangarevans used *marae* in the sense of sacrifice or first fruits because sacrifices and first fruits were made on the village green. The presence of pyramids on the green is only natural since kings' and chiefs' tombs were often pyramidal, so it was at least in Tonga, and it is possible that the circular guest house in Samoa is derived from a pyramid.

The presence of solar emblems in Tahiti suggest that the *marae* was there dedicated to sun worship. How can we reconcile this with our equation? It is quite a simple matter. The Polynesian chiefs were certainly conceived as being of heavenly origin, or at least connected with the heavens. In Hawaii and elsewhere chiefs' names often contain the word "sky." In Tonga the king's head and his tomb were called a "sky." In Samoa if a chief died they said "The sky has fallen." In Tahiti "sky" was the title of the highest chief or king. In Hawaii it was also the title of a high chief. It is needless to multiply evidence. One fact from Tahiti suggests that the kings were more particularly connected with the sun, for on the transference of the king's temporal power it was said, "The *Ra* (sun) has set."

Everything fits in with implacable logic. The *marae* are merely one detail in the institution of divine kingship; and we may have the more confidence in our conclusions as they outline a system of beliefs which

¹ *American Anthropologist*, (n. s.), vol. 17, 1915, p. 631-646.

curiously resembles what we know to have existed in Egypt: solar kings who are buried in pyramids and who are sometimes conceived as descended from the embrace of Heaven and Earth.¹

On the other hand the *nananga* were certainly not connected with divine kingship. Scanty as is the evidence, I think we may affirm this with confidence. Divine chiefs do not appear in any of the accounts, such as they are, of the *Mbaki* festival. It is significant that the area covered by the *nananga* is precisely that where the divine kingship is weakest; its former presence can be inferred from legends and customs, but the divine chiefs themselves have practically disappeared from all but a few tribes, and I should be inclined to say that the *Mbaki* worshippers are precisely the people who broke down the institution.

The true equivalent of the *marae* in Fiji is the *rārā*. The *rārā* is the village green. Round it stood the temples and the houses of the nobility with their foundations in tiers like unfinished pyramids. In the case of temples a square hut with an extremely high roof completed the pyramid. In this green all feasts were held, and the food was offered up with prayers; the first fruits were piled up there for presentation to the gods or the chief. In many tribes these offerings were superintended by a hereditary herald known as the *Tu Rara* or Lord of the Green. In Rotuma this herald is known as the *Fu Mara'e*. Now if,

Tu Rara = *Fu Mara'e*;

and *Tu* = *Fu* (for *t* becomes *f* in Rotuman),²

then *Rara* = *Mara'e*.

This *rara* also exists where the *Mbaki* cult prevails, and is one of the proofs that they once had divine chiefs. Clearly if the *rara* represents the *marae*, the *nananga* cannot. The *nananga* can scarcely be connected with the sun. For no trace of solar worship can be found in that part of Fiji where the divine chieftainship still exists; the chiefs, though divine, are in no way connected with the sun, not even with the sky. It is hardly likely therefore that sun worship existed where the chieftainship had decayed still further.

All we know about the *mbaki* suggests a ghost cult or some form of spiritualism. This cult may have been originally derived from a solar cult or be modelled upon it, but the sun probably had no part in it. The *mbaki's* nearest affinity is perhaps the cult of "water sprites," "stone

¹ Tregear under *Rangi*, and Erman's *Handbook of Egyptian Religion*, p. 29.

² F. *mata* : R. *mafa*;

F. *mati* : R. *mafi*;

F. *oti* : R. *ofi*; etc.

gods," or "stoneheads," which has in fairly recent times overspread Fiji; indeed it is quite possible that these spiritualistic cults are merely the exoteric and public ceremonies of the *mbaki*; for the devotees of the *mbaki* after concluding their rites in the *nananga* came to give a public exhibition in the village green. The water sprites likewise came on to the village green after completing their initiation in the bush. This connection between the *Rara* and spiritualistic ceremonies is however quite accidental; there is no historical relationship between the *Rara* and the sprites; the green is an ancient institution, whereas spiritualism has but recently spread over eastern Fiji from the hills. The *Mbaki* worshippers and the spiritualists merely came to perform on the green because it was the scene of all ceremonies, dances, and feasts.

In Tahiti the secret society of the *Areoi* performed on the green; Dr. Rivers rashly concludes that the *marae* belongs to the *Areoi* and that consequently their cult was addressed to the sun. The example of Fiji shows us how unfounded is such a view, and rather suggests that the *Areoi* were intruders.

In our present state of knowledge it is safest to believe that as,
rara = *marae*,

so

water sprites (and *mbaki*?) = *Areoi*,

that in Tahiti as in Fiji secret societies holding spiritualistic cults have made use of the village green which belonged to the old established religion.

Here if you like we have a clear case of culture fusion: two different cycles of beliefs have come into contact, and interfered with one another. The village green belongs to the cycle of divine kingship, such as prevailed over a considerable part of the world. As that institution decayed in Polynesia the sacred green lost its intimate connection with the gods and their earthly representatives; it became the scene of all ceremonials, and offered its convenient space to the public performances of a newly imported cycle of beliefs, that of secret societies.

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POSTSCRIPT

A friend of mine was kind enough to criticize my analogy of the Christian Churches. "Different as all these sects may appear to an outsider," he objects, "on closer examination they are all found to have a common origin and possess certain beliefs in common." I quite agree but would point out that the style gives no clue to which particular beliefs happen to be common to all. Wherever we find a Gothic church

or indeed any church built upon the Christian plan we may safely conclude that it was used for a worship of Christian origin, but we cannot tell how much of the original worship has been preserved or how far it has been transformed.

Even so the similarity between the plan of the *nananga* and that of the *marae* is a very strong presumption, not to say a certainty, that the cult of the *Mbaki* and the ceremonies of the *marae* belong to the same family and must have certain points in common. What exactly those points are the plan alone will never reveal to us. The common origin of the *marae* and the *nananga* probably lies a long, long way back in history and the Lord only knows what vicissitudes they have undergone since their first parting, what dogmas they have lost and what acquired. Even the *rara* and the *marae* which are very closely related indeed (the *rara* may be described as a very recent variety of the *marae*) have points of difference. The *rara* shows no trace of that solar worship which is faintly indicated in the *marae*; but how much less likely then is the *nananga* to preserve a solar character.

The articles of faith which are common to the *rara* and the *nananga* are, so far as our present knowledge goes:

1. Worship of *Kalon*. This word means indifferently gods or ghosts. Originally there may have been no distinction between the two; there scarcely is at the present day, yet we may say that the *Kalon* of the *rara* were gods; those of the *nananga* probably common garden ghosts.

2. In the *nananga* was held the *Mbaki* or crop festival. The *rara* was the place to which the first fruits were brought for offering to the gods and to the chief. I have shown elsewhere that the divine chieftainship was intimately connected with the crops.

Further study may reveal more points in common, but we shall have to be careful to distinguish those they owe to their common origin and those they have borrowed from one another; for having met again after a long period of separation they must inevitably have influenced one another, like two stars broken off from the same mass long ago which once again cross one another's orbit.

A. M. H.

PRACTICES AND CUSTOMS OF THE AFRICAN NATIVES INVOLVING DENTAL PROCEDURE¹

DENTAL PRACTICES among the African natives are divided into two classes, one being for relief of pain, the other a matter of custom. The

¹ Abstract from an article in the *Journal of the Allied Dental Societies*, New York, March 18, vol. XXIII, p. 1.

latter is the most important. Each tribe has its own dentist and its own mode of procedure. These operators are influential and, as a rule, wealthier members of the tribe. Each tribe has its own remedies, evolved in the course of time by custom and experience. The methods involved for tooth extraction are all very crude and cause tremendous sufferings patiently borne by the subject, when voluntarily submitting himself for the operation.

The mutilation practices involving the teeth vary in locality and also in style, as can be readily proven by an examination of the maps and the exhibits in the article. If one tribe has more than one style it could not be proven. The most difficult methods—the serrate or saw style, buccal horizontal relief, mesial incisal, and medial triangle filing—are the least practiced, while on the other hand, the pointed filing being the easiest is much more in use. This fact may also bear a direct relation to utility and origin emphasized in the following paragraph. The filing is done either by first chipping piece by piece and smoothing down the rough edges afterwards or by direct filing from start to finish. This is done by special members of the tribe.

The filing to a point, however, seems to be a predominating custom and is probably the predecessor of all the other methods. The latter, I believe, are changes adopted by one or more tribes in one given locality and in this the tribes around Lake Nyasa are the foremost, having seven methods to their credit. The natives of these regions are superior in many ways to the other tribes which probably explains their originality in the different methods employed.

No proof has so far been established that filing bears any relation to cannibalism, although this theory has been brought forward many times. The writer believes in the cannibal hypothesis, first, because some of the cannibal tribes do file their teeth to a point; second, because they are great meat eaters and pointed incisors would be of a distinct advantage; and third, because the non-cannibalistic tribes who do file their teeth can give no reasons at all for the custom, but just state that it is a custom of the tribe.

The place of origin of these mutilations has not been proven as yet, but the writer believes that it started in the Congo region. Of the three tribes who have this custom as a probable remnant of cannibalism, two are located in the Congo and one in Nigeria. The reasons given by the other tribes for this custom are mostly unknown to them, and those who have any reason differ as to the origin of this habit. Two regions may be selected as the place of origin of the idea of tooth mutilations:

First, equatorial Africa, second. Lake Nyasa district. The latter having seven methods to its credit might be considered the birthplace, although four of these cannot be adjuncts to the eating of meat but are simply for ornament. In considering the distribution of the custom, the Lake Nyasa district is found to be the most southern occurrence of tooth mutilations. If the tribes there were the originators one would expect the custom to appear all around them. This is not the case. There is but one custom appearing south of this district practiced by but one tribe. To this we may add that the Bantu tribes are known to have migrated southward. These facts, taken all together, point out that this region was not the original home of the mutilations.

Equatorial Africa has also seven customs to its credit, but these are not restricted to a small area, since they have spread in all directions, though more southward than northward. This fact considered in connection with the southward migrations of the Bantu, might tend to prove that we have here either the cradle of the custom or that it was in vogue here at an earlier date than in the Lake Nyasa district.

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PUEBLO CLANS: A REPLY¹

PROFESSOR KROEBER makes my "theory" entirely too definite. I do not know how much truth there is in the Hopi migration legends, but I do not reject them *in toto* merely because they are legends. On the contrary I consider that they embody some truth because other facts—the Shoshonean language and Pueblo culture of the Hopi, and the distribution of clans among the Hopi and Zuñi and in Cochiti—tend to bear them out. I agree with Professor Kroeber that it is most reasonable to assume that the core of the Hopi nation was Shoshonean, and that the majority (or perhaps I should say the plurality) always spoke a Shoshonean dialect, but this does not preclude the incorporation of other peoples, even in considerable numbers, from a very early date, nor the further possibility that such incorporations might yield in time a mass of population greater than the descendants of the original nucleus. The Lower Creeks may be cited as an actual instance of this process, only two of their ten or twelve towns having originally spoken the now universal Muskogee idiom.

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¹ Pages 328-331, above.

ANTHROPOLOGICAL NOTES

THE SOUTHWEST SOCIETY

STEPS toward the formation of a society which would unify the anthropological interests in the Southwest were taken during November and December at the weekly luncheons of the New York anthropologists. Dr. Elsie Clews Parsons, who initiated the movement, consulted, either personally or by letter, the principal workers in this field. A committee to draft a constitution reported a brief document which was approved by the organizing committee and follows:

CONSTITUTION

ARTICLE I.—*Name*

This society shall be named the Southwest Society.

ARTICLE II.—*Objects*

The objects of the society are to promote inquiries into the culture of the peoples of the southwest through field work undertaken independently or, whenever possible, in coöperation with other institutions; and to arrange for the publication of the material collected.

ARTICLE III.—*Membership and Dues*

The society shall consist of members paying annual dues of \$1.00. Members may pay dues in advance for any term of years.

ARTICLE IV.—*Officers*

Section 1. The officers of the society shall be President, Vice-president, and a Secretary-Treasurer, and two Councillors. Collectively they shall constitute an Executive Committee.

Section 2. The Executive Committee may at any time during the year add to its number a representative from any coöperating institution.

ARTICLE V.—*Meetings*

Section 1. There shall be two stated meetings of the society, a winter meeting held in conjunction with the annual meeting of the American Anthropological Association, and a summer meeting held in

the southwest, the specific place and the time to be subject to the convenience of the members resident or sojourning in the southwest.

Section 2. Special meetings may be called by the Secretary at the request of the Executive Committee or of nine members of the society.

Section 3. Notices for special and other meetings shall be sent by mail at least fourteen days prior to the time of holding the meeting, and a notice so sent shall be considered sufficient notification.

Section 4. At the winter meeting the officers of the society shall be elected, and reports shall be made by the President and Secretary-Treasurer. These reports shall be printed and distributed to the members. At the summer meeting reports shall be made by the President and Secretary-Treasurer and, when possible, an account shall be given of the field-work in hand.

ARTICLE VI.—*Quorum*

Nine members present at the meetings of the society shall constitute a quorum. A majority of the Executive Committee shall constitute a quorum.

ARTICLE VII.—*Amendments*

The constitution and by-laws may be repealed, amended or added to by a vote of two-thirds of the members present at any meeting provided twenty-days' notice in writing of such proposed amendment be given.

BY-LAWS

ARTICLE I.—*Business Year*

The business year of the society shall commence on January 1 and end on December 31.

ARTICLE II.—*Elections*

The officers shall be elected by ballot to serve for one year. The Executive Committee may fill vacancies.

ARTICLE III.—*Publication*

The Executive Committee shall arrange for the publication of material. If within one year of the termination of field-work the material is not ready for publication, the material shall pass into the hands of the Executive Committee except in so far as rights may be reserved by the coöperating institution or institutions. The Executive Committee may extend the time allowed for the preparation of manuscript.

THE ANNUAL MEETING

The annual meeting at Baltimore was fairly attended and proved to be both enjoyable and profitable. In the absence of President Kroeber, Vice-President Swanton presided at the sessions of the Anthropological Association. Dr. J. W. Fewkes, ranking Vice-President of the Folk-Lore Society, presided at the annual meeting of that society in the absence of the President, C. M. Barbeau, who was taken ill in New York on his way to the meeting.

In addition to the regular sessions devoted to the necessary business, to the reading of papers and their discussions, there were enjoyable luncheon groups and Friday evening a dinner at the Southern Hotel. This dinner was attended by the following members, Boas, Goddard, Lowie, MacCurdy, Merwin, Michelson, Parsons, Pearce, Speck, Swanton, and Wissler. Prof. Boas entertained the company with amusing illustrations of his proposed adaptation of binomial Latin nomenclature to ethnological discussions. The official report of the sessions will appear in the next number.

FATHER PACIFIQUE, missionary at Sainte Anne de Restigouche, Province of Quebec, expects to issue shortly a new edition of Father Kauder's Book of Prayers in Micmac characters. This system is believed to have been devised more than two centuries ago by Father Le Clercq, but was later improved by Father Maillard and reduced to print in 1866 by Father Kauder. The original edition of Kauder's work was largely lost by a shipwreck, so that the remaining copies are now quite rare. The new edition will reproduce Kauder's work in facsimile, but Father Pacifique will prefix French headings where they now appear in German. The cost of the new edition will be two dollars.

THE folklore and ethnographic survey, by Mr. C. M. Barbeau, of some groups of French peasants in Temiscouata and Gaspé counties, Quebec, from June to October, 1918, has contributed considerably to increase the folk-materials already in the possession of the Anthropological Division of the Geological Survey of Canada. The new collections include over 1,300 folk-songs; 60 folk-dances for instruments; 17 long folk-tales; 28 anecdotes bearing on were-wolves, spirits, apparitions, and various ancient beliefs; 280 photographs of objects and structures connected with folk-ethnography; and extensive notes on various related topics.

PROFESSOR ROLAND B. DIXON, ex-President of the Association and head of the department of Anthropology at Harvard University, is a member of the peace delegation. Professor Dixon's general knowledge of ethnology and his special and long continued interest in Russia and Asia make his selection an ideal one.

CAPTAIN W. C. FARABEE, acting Secretary and Treasurer of the Association, is in attendance at the Peace conference.

DR. TRUMAN MICHELSON visited the Fox Indians at Tama, Iowa, during the summer. He is lecturing at George Washington University.

DR. WALTER HOUGH, of the National Museum, made an archaeological trip to the upper Salt river drainage last summer.

DR. ALEŠ HRDLIČKA, of the U. S. National Museum, spent the month of November in an anthropological reconnaissance of southwestern Florida.

MR. JAMES MOONEY, of the Bureau of American Ethnology, has returned to Washington from several months spent in field work among the Kiowa Indians of Oklahoma.

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